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Certainly it is excellent discipline for an author to feel that he must say all he has to say in the fewest possible words, or his reader is sure to skip them; and in the plainest possible words, or his reader will certainly misunderstand them. Generally, also, a downright fact may be told in a plain way; and we want downright facts at present more than any thing else.—RUSKIN.

Original Communications.

SYMPATHETIC OPHTHALMIA.*

BY W. CHEATHAM, M. D.

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Having had lately several recoveries of sympathetic ophthalmia, I think them rare enough to report. Some recent writers have accused the ciliary nerves, the circulation of the blood, the optic nerves, etc., each as the channel through which a diseased eye may affect its fellow. My own belief is that sympathetic ophthalmia is produced through the same channel by which defective refraction may produce headaches, bad teeth cause diseased eyes, a diseased eye produce epilepsy, a diseased uterus or stomach cause weak and inflamed eyes, that is, through the sympathetic system of nerves.

Inflammation in the fellow-eye may start in any portion of it, either optic nerve, retina, choroid, iris, ciliary body, cornea, or conjunctiva, yet it depends upon this sympathetic nerve for its cause. I explain it as sympathetic irritation is explained; this sympathetic inflammation simply being the result of continued sympathetic irritation. I should mention that as all diseases lately are claimed to have their own

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have their own micrococcus, one has been ascribed to this disease, though I fear with not as much justice as the micrococcus of delirium tremens has been called "the worm of the still."

A writer in the last Archives of Ophthalmology, after going pretty thoroughly over the various theories, comes, I think, justly to the following conclusions :

1. That the doctrine that sympathetic ophthalmia is the result of progressive optic neuritis, which spreads from the primarily to the secondarily affected eye, is supported by no facts worthy of serious consideration, but is the outgrowth of grossly misinterpreted clinical, pathological, and anatomical observations.

2. That the septic-choroiditis theory of Leber rests upon evidence which is scarcely more substantial, and from both an anatomical and clinical point of view is open to serious objections.

3. That there is an overwhelming preponderance of testimony, both clinical and pathologico-anatomical, in favor of the one-at-a-time universally accepted doctrine that sympathetic ophthalmia, like sympathetic irritation, is a reflex neurosis depending upon irritation of the ciliary nerves; and that as this testimony goes far to show that inflammatory changes of texture as well as abnormalities of sensation may be produced by reflex-nerve influence, it has a very important bearing upon the theory of the etiology of inflammation at present most in favor.

In speaking, a moment ago, of the effect on other organs of sympathetic irritation arising in some distant organ, I mentioned epilepsy as resulting from a disorganized, painful eye. I had one such case, in which the fits were as frequent as ten or twelve a day, that was relieved entirely by the removal of the diseased eye.

I have in the last twelve months seen four cases of sympathetic ophthalmia. Wounds of the cornea, in which the iris becomes engaged, and wounds of the ciliary region, when pressure upon the ciliary nerves is made by a cicatrix or by plastic

material are much more liable to produce inflammation of the fellow-eye than other injuries. This sympathetic inflammation may set in at any time from two weeks to many years after the injury. I have seen it forty-seven years after the primary trauma. I fear, from what I have said, I have left the impression that a blow on, or wound of, the eye is necessary to produce this affection in the fellow-eye; almost any disease of an eye may endanger its companion by this sympathetic neurosis.

Wounds of the eye the result of operations, such as extractions of cataract or iridectomies, may endanger the fellow-eye. It is not necessary that the diseased eye should be painful of itself, or from pressure, to produce disease of the fellow-eye. The symptoms of sympathetic irritation which, I contend, always precede inflammation, are slight temporary blurring of vision, fear of light, flashes of light, pupil fails to dilate and contract as normally, and easy fatigue of eyes from use.

Just here I will report four cases seen lately:

Mr. P., struck in the left eye with a nail six months before he came to me. The nail made a large wound of sclera in ciliary region. Four months after the injury, symptoms of sympathetic irritation set in, which were soon followed by plastic iridocyclitis. When I first saw him, vision of both eyes was zero. I advised removal of wounded eye, which was submitted to. By leeching, warm and cold cloths, iodide of potash and mercurial inunction continued for four months, he returned home with vision enough in right eye to go any where he wished. He afterward made frequent visits to see me, driving forty miles alone.

Capt. S. lost sight of the left eye, some years ago, from inflammation. Cornea of the left eye opaque, synechia posterior, with cataract. Some months ago sight of right eye begun to fail. When I saw him he could only distinguish daylight from darkness; left eye was not sensitive to either light or touch. After trying for a few days treatment with no change for the better, I enucleated left eye, fearing the trouble was sympathetic.

The eye was removed under local anesthesia. He was now put on the mercurial inunction. I forgot to say the inflammation in the right eye was plastic, and appeared as if it was going on to suppuration. Leeches were applied to temple; room darkened and rest in bed advised; cold cloths applied. In eight or ten weeks the captain returned to his work with vision $\frac{2}{10}$; synechia posterior.

Mr. C. had choroiditis, left eye; was treated by another oculist who performed iridectomy. Iridocyclitis followed, with closure of pupil. Some weeks afterward a spot of choroiditis started in right eye in supero-temporal quadrant. His oculist advised iridectomy in this eye, with enucleation of left; against which the patient kicked, and fell into my hands. I treated him for some months, getting vision $\frac{2}{10}$, right eye. No vision of course in left eye. He returned home, reporting to me only occasionally. About four months ago he returned with symptoms of inflammation—right eye again; vision $\frac{2}{10}$; neuro-retinitis, right eye. I advised enucleation of left, to which he readily acceded. In three days the neuro-retinitis was greatly improved and vision doubled. He has gone on improving slowly but steadily since.

Mr. N. was struck in right eye, back of ciliary region, by a piece of steel about one third of an inch square, laying open the vitreous chamber, evacuating most of vitreous humor. I put him through the usual treatment, but sympathetic irritation soon followed, then inflammation. I advised enucleation, when he first came to me, and insisted upon it now; but, being an ignorant fellow, he said he would just as soon have his head taken off. I treated him as usual in these cases, with no good result. He lost both eyes. This was an especially sad case, as he was a very poor man with a large family. None of these cases gave any syphilitic history. I think they make a pretty good showing of what can be done in such cases, when carefully and properly managed. It shows also that these advisers of early enucleation, or rather advisers of enucleation of all eyes that are blind are of no service, because if sympathetic inflammation sets

in all is lost, are sometimes mistaken. My views as to this point will be thoroughly given a little later.

An extremely important point to answer is, when an injured eye presents, shall we enucleate or not?

Two years ago a mechanic came to my office, with a wound of sclera, retina, choroid, and ciliary body. The wound was fully half an inch long, extending from near temporal edge of inferior rectus, up and in, to near superior edge of internal rectus; there was great loss of vitreous. I cleared the edges of the wound thoroughly and put in one silver suture near the center, brought the edges together. Used atropia sulph. and cold cloths. In four days the man could tell the time of day by a watch. He made a rapid recovery.

I was called to Charlestown, Ind., two or three years ago, to see a lady. While sewing on an old umbrella, the needle struck one of the steel ribs and, rebounding, struck her on the cornea of left eye. It gave her scarcely any pain at the time. I saw her one week later, when suppurative ophthalmitis had destroyed the organ.

Here we have two cases illustrating the uncertainty of the result of injuries of the eyes. Shall we then enucleate all blind eyes, and all injured eyes where we feel no sight can result from treatment, because of this danger of sympathetic ophthalmia? In the first place no one can tell at once whether or not a majority of the injured eyes will go blind. In considering this subject let us first decide as to whether this operation of enucleation is, as most physicians believe it to be, a harmless one. Graefe, Mannhardt, Horner, Just, Pagenstecher, Verneuil, Vignaux, and Mauthner, report fatal cases, that is, death the result of enucleation. I myself have had severe hemorrhage to follow enucleation in two cases; in one case nearly fatal. In the young it often leads to deformity, by contraction of the orbit. Again, sympathetic ophthalmia is occasionally excited by enucleating the diseased eye. Again, when the sympathetic ophthalmia is a serous iritis, the enucleation of the offending eye excites a plastic iridocyclitis with loss of the good eye. So it

will be observed that enucleation is not entirely harmless. The picture on the other side (that is, the danger of total blindness) is not very bright, I admit, yet these small matters must be taken into consideration.

Is it proper to advise the enucleation of a hopelessly blind eye as long as the other is normal? I can better illustrate my answer to this by relating two cases bearing on the point, and again referring to one I mentioned a few moments ago:

During last winter a young man came to me from Clarks-ville, Tenn., having been shot in his right eye the day previous. The shot passed through cornea, iris, lens, vitreous, retina, choro-roid, and sclera, into the back of orbit. Of course the latter was not discovered until after enucleation. The young man was a farmer in medium circumstances. I made the following statement to him: "If you will stay here under my observation for some weeks, it is possible that I might save that eyeball for you, and possibly even with some vision. There will be great loss of time and considerable expense; notwithstanding the best treatment I can give you, it is quite possible that the eye may finally have to be removed. There are many chances for this, with some risk of its affecting the other eye, and, even if it does not destroy it, damaging it very much." After telling him such objections to an enucleation, its dangers, etc., I told him of the discomfort of glass eyes, besides their being expensive. "Again, if you decide to have it enucleated, I can do it at a certain time, at such a price, and you can return home the same day, or you can stay a week or ten days and get a glass eye, or, if you prefer, go and come back in ten days and get it. One great objection to your endeavoring to retain this eye is the distance you are from an oculist. Should sympathetic inflammation set in, it might destroy your good eye before you can reach an oculist." He wisely decided to have the eye removed, which was done next day. The condition of the eye, shown after enucleation, confirmed the wisdom of his decision.

A gentleman, living in Louisville, received a shot in his left

eye, in about the same location as in the other case. The same statement was made to him, modified perhaps by rather more hope for vision. In this I found I was mistaken, as the eye is hopelessly blind. But the main differences between the two are these: Number two is in a better financial condition, can better stand expense and loss of time; and is where he can find an oculist at any minute almost. Another important point bearing on this portion of my paper, let me recall the case mentioned a moment ago, where both eyes were lost as the result of trauma to one. Here I advised immediate enucleation, first, because he was poor and could not afford to lose the time involved in my efforts to save it, but mainly because of his ignorance, fearing his inability on this account to recognize sympathetic irritation. He did fail, not only to recognize sympathetic irritation but also sympathetic inflammation. As a result he is totally blind.

So it will be observed that this question of preventive enucleation hinges on other important points beside the dangers of sympathetic ophthalmia: The patient's financial condition—I mean his ability to lose the time for a lengthy treatment—the nearness of a physician able to recognize sympathetic irritation and enucleate the offending eye, and the intelligence of your patient, are very important factors to be considered.

So far I have only spoken of cases in which the offending eye is blind. Here my advice is, if the patient is intelligent enough to recognize sympathetic irritation, and living in close proximity to a competent physician, do not enucleate unless such irritation declares itself. But if the person be either ignorant or too poor to afford the loss of time needed in preventive treatment, or in one living miles from competent medical advice, immediate enucleation is indicated.

Again, if the sympathetic inflammation takes that type known as serous iritis, an enucleation should be avoided if possible, as such a procedure often results in a plastic iridocyclitis, with loss of the good eye.

I have yet said nothing of the enucleation of injured eyes

with some useful vision, or of eyes in which useful vision can be restored by either treatment or operative procedure, such eyes exciting sympathetic inflammation. "Never enucleate such an eye," so says Mauthner, "when the sympathetic inflammation takes that form known as iridocyclitis." He says this, believing that any eye with sympathetic iridocyclitis is surely lost. In this paper I report two recoveries. In one case there was only perception of light; in the other vision was zero; one got vision enough to go any where he wished; the other has vision= $\frac{3}{4}$. Mauthner gives this advice, believing the sympathetically affected eye is sure to go, and expecting to depend upon the primarily affected eye for vision. If sympathetic iridocyclitis is not, then, as grave a disease as he thinks it, and as my two cases out of three indicate, can we follow his advice? His creed is this—I quote him because he has studied more thoroughly and written the most comprehensive work I know of on this subject: "Enucleation may be performed as a preventive. It must be performed in the stage of irritation. It can not be performed in iritis serosa and iritis plastica. It can be performed in iridocyclitis-plastica, provided the eye causing sympathy is totally blind, but not in a state of violent irritation." In commenting, then, on this creed of Mauthner's, I would modify it; and I think the three cases of iridocyclitis reported by me, with one partial recovery, and one good recovery—and in the third case we can not tell what might have been the result if he had reported to me sooner, even if he had done so as late as the onset of sympathetic inflammation, and I think the result in the other two cases justifies me in thus inserting a question mark—support me in this modification. I give this as my modification of Mauthner's creed: Enucleation may be performed as a preventive under the restrictions before laid down. It is better to perform it in the stage of irritation; but if you can not perform it then, in the stage of inflammation, for while there is life in the eye there is hope. It should not be performed in iritis serosa. It can be performed at the onset of iridocyclitis-plastica, although the eye causing sympathy may not be blind.

MISTAKEN DIAGNOSIS—ALMOST A LAPAROTOMY.*

BY B. W. STONE, M. D.

The record I now read is made from notes taken of the case, which occurred some years ago:

A German woman, aged thirty-five years, ignorant, totally unacquainted with the English language, was transferred to a metropolitan hospital from the county almshouse for treatment for abdominal dropsy.

The history of the case, as given by the almshouse physician (the woman herself could give none) was that the woman was admitted at the almshouse, on account of abdominal dropsy, five weeks ago. Paracentesis was performed about one inch below the umbilicus, and about five gallons of fluid withdrawn. She has three children, and has had two abortions, one at third and the other at the fifth month of gestation, the last occurring three years ago. She has since given birth to a full term-child, now living, about seventeen months old. During the past winter she exposed herself to cold and damp a great deal; was always thinly clad, and frequently did washing and out-door work in her bare feet during quite cold weather, and suffered with a constant bronchial catarrh. Her abdominal trouble began six months ago. She was suddenly seized with sharp pains in her back and side; these continued for a week without mitigation, when an enlargement of her abdomen became distinctly perceptible. Subsequently, vomiting occurred at intervals, continuing for two months. During this time the abdominal enlargement rapidly increased. Five weeks ago she was admitted into the almshouse in this condition. After the tapping, which took place ten days ago, she was transferred to the hospital, with no other account of her case or condition than is recorded in this statement.

With the above history before him a well-informed physician, but only a yearling in practice, made the first examination—a

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very cursory one—of the case, and pronounced it simple ascites. Two accomplished physicians, and of larger experience, were called into consultation. They made a protracted examination and easily found a large tumor, strangely overlooked by the first examiner, within the abdominal cavity, apparently having its origin in the left side of the pelvis, and rising to within an inch and a half of the umbilicus. Influenced by the account of the tapping in the case, excluding ascites, they pronounced it left ovarian dropsy. Hydragogues, diuretics, and diaphoretics were ordered, with the application of the ointment of iodide of potassium over the site of the tumor. One week subsequently the case was turned over to a distinguished professor of surgery for operation.

The professor made examinations with the sound and speculum, and externally, by palpation, etc., inquired into the history of the case, and finally concurred in the diagnosis of ovarian dropsy. Next day an eminent physician of very large practice was called to examine the case. The sound and speculum were brought into requisition, and careful external examination also was made. He also concurred in the diagnosis of ovarian dropsy, and as to the advisability of early operation. A few minutes after this examination a slight bearing-down pain expelled a large clot of blood. This was considered of no consequence, as a small opiate soon removed all suspicious symptoms. It was decided to perform ovariectomy on the fifth day after this examination. The day following, the professor delivered a clinical lecture to a large class of medical students upon ovarian disease, the patient being before them. The points differentiating the disease from other conditions were carefully explained, and the announcement of the operation made.

An able obstetrician, a professor and an author of national reputation, was finally called in, late in the afternoon of the day immediately preceding that set for the operation. This careful specialist listened to the history of the case and all previous examinations, and proceeded to examine the tumor, first externally, by palpation, percussion, recognizing co-existing ascites.

Upon using the sound, that instrument passed to a depth of five and one half inches into the cavity of the uterus. He diagnosed the existence of two tumors; one, the enlarged and subinvolutus uterus, the other, of doubtful character, but lying closely upon the womb, probably a cystic tumor developed within the broad ligament or in the mesentery.

The sound, when withdrawn after this exploration, as when it was introduced by those who had examined the patient previously, was found covered with blood, and, as in former examinations, pains came on soon afterward. This time the pains soon assumed a more violent bearing-down character, for which ten grains of Dover's powder was given. In fifteen minutes after the examination the patient passed, from her vagina, three pints of blood and water. Some little bleeding continuing after this, lumps of ice were passed up the vagina, which apparently arrested it.

The patient gave birth, at half-past eight o'clock, P. M., to a six and one half months' fetus, which lived until six o'clock the following morning, and died.

The candid professor lectured, the same day, with the tumor before him, upon ovarian tumors, the various methods of diagnosing them, etc.

Four minute ecchymotic spots on the back of the child doubtless indicated the points where the sound was arrested in the last examination, and supposed then to have been the limit of the uterine cavity. Attention is called to several circumstances calculated to mislead the most careful: (1) The account of the actual existence of some form of dropsy; (2) The fact that the physicians who made the earlier examinations were those of the least experience, and though their explorations were not as thorough as the gravity of the case demanded, their opinions powerfully biased subsequent diagnosis, and doubtless rendered less thorough each successive examination. It is also worthy of remark that neither ballottement nor auscultation were employed by any of the physicians in the case.

REPORT ON EPIDEMICS, 1883-4.*

BY J. M. HARWOOD, M. D.

The answers to letters of inquiry concerning epidemics sent to nearly every county in the State enable me to make the gratifying statement that Kentucky has in large degree escaped epidemics of the more commonly fatal diseases. But nine counties have reported epidemics that were considered worthy of being so called. Of these but three are noted for either fatality or extent, the remainder being worthy of mention solely because of the small fatality and narrow limits accupied by the diseases.

Mercer County reports the most extensive epidemic of measles of a mild form, about five hundred cases with five or six deaths; twelve cases of typhoid fever, two deaths.

In Warren, pertussis prevailed very generally with no death at date of report.

In Fleming County there were forty-five cases of scarlatina, with seven deaths. Nothing peculiar or unusual marked the disease. Also an epidemic of measles of mild form; no deaths.

Boyle County reported ten cases of typhoid fever in and around Junction City. The sanitary condition of the place being bad, and the drinking-water, which is procured from shallow wells, being worse.

In the Deaf and Dumb Asylum, at Danville, three cases of scarlatina appeared simultaneously. These were promptly isolated, and no more occurred.

Clay County reports an epidemic of pertussis, with no deaths; also some dysentery scattered over considerable territory, a few deaths; no evidence of contagion, or of its having a common origin.

The most fatal as well as most extensive epidemics appeared in Knox and Graves counties. In the southwestern portion of Knox County, over a territory of about twenty-five miles in length, nearly every family was afflicted with dysentery, and

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the fatality amounted to about fifty per cent. The number of cases is not reported. The epidemic commenced about the middle of June, and lasted until the last of October. Preceding this outbreak was an epidemic of typho-malarial fever, confined principally to the same locality. This was also very fatal. The region is low and marshy, the water standing on the surface during wet weather. The drinking-water is usually obtained from shallow wells which go dry in summer, and when the fall rains set in are often the receptacles of filth.

A most extensive epidemic of dysentery is reported in Graves County during last summer. It commenced in Mayfield, and gradually spread over the whole county. The sanitary condition of the town is reported as having been bad. The greatest mortality was among children, though all classes and all ages suffered. One peculiarity of this epidemic was that the fatal cases invariably had a cold stage every morning, which often ended in death. Quinine appears to have been the most successful remedy. The "ipecac treatment" was not successful in these cases.

Several slight epidemics are reported from Paducah, the worst of which was that of dysentery last summer. It was of that type known as "bilious." The patient was generally taken at night with profuse diarrhea; this was followed in ten or twelve hours by hemorrhage and vomiting, which continued through the disease. The stools and fluids vomited were bilious in appearance. The tongue remained red and without coat during the whole course of the disease. The number of cases or percentage of deaths has not been reported.

In Shelby County, smallpox prevailed as an epidemic from the latter part of January until May 1st. Sixty-five cases, fifteen deaths. But one white person suffered; the remainder were colored. The lack of a pest-house—a building in which the sufferers could be isolated—led to the long continuance of the disease. On the first of April a suitable building was obtained and the disease was brought quickly under control. In the neighborhood of Simpsonville, Shelby County, last fall,

there were about twenty-five cases of diphtheria, with six deaths. In endeavoring to trace the origin of this disease it was found not to result from contagion but made its appearance in those houses whose sanitary condition was bad. Dr. Ryon, who treated most of the cases, is of opinion that it came from bad water.

SHELBYVILLE, KY.

A VESICAL CALCULUS VOIDED BY RECTUM.*

BY J. F. M'ELROY, M. D.

A colored boy, fifteen years old, had suffered for several years with symptoms of stone in the bladder. He was very weak and much emaciated. The urethra was too sensitive to allow the passage of a sound. I endeavored to improve his general health, and put him if possible in condition for an operation. A week after, while at stool, he passed by the rectum a mulberry-colored calculus two inches long, nearly an inch in its short diameter, and weighing a little under one ounce. A rectal examination revealed the opening between the bladder and the gut by which the stone had passed. The patient improved at once and rapidly, and in three months was well. While on his feet the urine came naturally by the urethra; when in the recumbent posture it came by the rectum. A year later the patient died of malarial fever.

The autopsy, made by Dr. McCormack and myself, revealed an opening between the bladder and rectum large enough to admit the finger. The walls of the bladder were thickened, and the mucous membrane of the rectum was much congested.

BOWLING GREEN, KY.

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BIGELOW'S LITHOPLAXY.*

BY A. W. JOHNSTONE, M.D.

It seems difficult for the ordinary run of practitioners to recognize the fact that a stone is never the disease itself, but is always a symptom and a very bad complication of a great many different conditions; consequently, when a stone is found, they think that with its removal their responsibility ends; and when a stricture, an enlarged prostate, an inflammation of the ejaculatory ducts, or any other organic interference with the urinary tract is discovered, they jump at the conclusion that this is the "*fons et origo*," and leave the unsuspected stone to do its work.

If I can convince you that, no matter how satisfactory an organic cause you may find for a vesical disturbance, over and above it there is always a possibility of a stone complicating the known trouble, this digression from our subject will be time well spent.

Your suspicions once aroused, Thompson's searcher, with its sharp curve and short beak, if properly handled, will find most any stone, though it may not give a satisfactory click from a soft phosphatic mass. I have found that Cammen's stethoscope will magnify this sound so as to make it quite satisfactory. If you are accustomed to this and will auscult the vesical region of the abdomen with it while you make the point of the sound tap the suspicious point in the bladder, it will clear up many a doubt. The difference between the sound passing over a hard stone, in a sacculated viscus, and a soft phosphatic stone is at once made plain, and the information it gives about the condition of the vesical wall is not to be despised. A uric or oxalic stone will speak for itself, but if you once get used to the tones that the common stethoscope gives you will not be without it.

Stones, however, are not invariably found, though the most careful search has been made. Prof. Sands told me he had once

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failed to find a stone which was afterward crushed by a surgeon who himself sounded eleven times before finding it.

How to remove the stone is a not less important question than how to find it. Bryant says that in children we do not want a much safer procedure than to cut, but "in adults the danger of lithotomy rapidly increases with age, the mortality being one in eight between seventeen and forty, one in four above that age." Holmes, Gant, Erichsen, Ashhurst, Thompson, Gross, and Gouley differ but slightly from the foregoing.

Until the birth of lithoplaxy, the best that crushing could do in spite of the most carefully selected cases was about one death in seventeen, by Mr. Keith, of Aberdeen. The next best by Sir H. Thompson, on a much larger series, one in about sixteen. Whereas, since Bigelow's ideas have been put in practice, the average death-rate of ordinarily good operators, without much selection of cases, has been about one in thirty-three and one third. It is true that there are American lithotrists who will dispute these statistics, but when you come to sift their lists you will find that they embrace a great many children, and that all were done under the best possible antiseptics, plenty of fresh air, in private practice, and on selected cases.

In old times only small stones were crushed and in patients with no renal complications. Now, however, we hear of stones being successfully aspirated that weigh from five hundred to one thousand grains, and even more. Formerly it was with fear that a medium-sized uric stone was attacked, but now we do not hesitate on a good large mulberry, and instead of the weeks and even months that we used to keep working at the unfortunate, we expect to send him home safe and sound within ten days. We now care little for a cystitis, and we never wait on nephritis if we are sure it is not an old purulent affair. Strictures and enlarged prostates serve only to make us a little more careful in the operation. When Bigelow demonstrated that with a full sized catheter the bladder would stand the requisite amount of gentle handling necessary to evacuate its contents provided no solid material was left for it to wound itself on,

surgeons of New York and Boston, London, Paris, and indeed the rest of the civilized world except our own country west of New York, were quick to adopt the method. Why is it, then, that the operation has made such slow progress with us? The only reason I can give for it is the proverbial dislike we have of details, and this, above all others, is the operation for the painstaking surgeon. The saying that "the day of brilliant surgery is over," has long since become threadbare, but I am sure that no operation proves it more thoroughly than this. By personal experience with most of the recognized surgical procedures I can testify that there is no operation that takes more care and forethought, and when duly exercised there is none that gives more satisfactory results.

On the 6th of last April, assisted by Drs. Howe and McClure, of Shelby City, Boyle, of Danville, Carpenter and Payton, of Stanford, I removed, from a gentleman's bladder, three hundred grains of phosphatic stuff. He was sixty-seven years of age, and for some years had been the subject of occasional spells of phosphatic cystitis from the residual urine of an enlarged prostate. The attack which resulted in the finding of the stone had existed since November. On measurement it was found that Otis's rule called for thirty French, which the urethrometer gave every where except at the meatus, where it was twenty-four. This was therefore split and allowed to heal before the final operation. After the first introduction of Bigelow's medium-sized lithotrite under anesthetic, it was found that twenty-six was the largest catheter that would pass the bar of the prostate, and, as the only one of that number I had was curved, it made the evacuation necessarily slow. The crusher and tube were introduced twice each, and would have been once more had it not been that the heart began to get restless under the A. C. E. mixture. Since the operation the patient has passed, all told, about thirty or forty grains of fine sand, and four pieces that were too large to pass the caliber of the twenty-six, but none of them would have been missed by a straight thirty. In spite of a pneumonia which he developed a few days after the operation

his cystitis is about well, his micturition from every few minutes has gotten down to once or twice a night. He has gained flesh and strength rapidly, and from being confined to room and bed he now goes where he pleases, and does pretty much what he likes. There may still be a small fragment in the pocket behind the prostate. I can not remove his prostatic difficulty, and in spite of the most careful removal of his residual urine, we may expect an occasional deposit of phosphates, which will necessitate a careful watch being kept on him during the rest of his life. If vesical trouble shows itself, an occasional search will have to be made for small concretions.

Thompson says that he has cases under his care from which once or twice a year he removes a small ten-grain stone, and thus keeps them in a state of comparative comfort. The danger of this operation for so small a stone in a comparatively healthy man is little more than that of pulling a tooth.

During the aspiration of the fragments, I used on the same tube, first, Bigelow's original evacuator, and afterward Otis's improvement on it.

The objections to lithoplaxy are, first, the uncertainty in the selection of cases, and the accidents which come up while it is in progress. I have already indicated that there are very few adults for whom this procedure should not be selected. Of course, male children should always be cut, for the caliber of their urethras is never large enough to permit of sufficiently rapid evacuation, nor would it be safe to attack a medium-sized urate stone with the delicate lithotrite that would pass this small canal. I am sure that this is much the safest procedure for females of all ages. Where the ureters are dilated, or other complicating diseases of the urinary tract are present, lithoplaxy is far preferable to any other method of removing a calculus. The case I have just reported had not only a sub-acute desquamation of the larger tubes of the kidney, but also a few casts in his urine, all of which have now cleared up. Very large oxalate stones had best be left to the knife, unless the urethra will permit of an unusually powerful lithotrite. Of

course, sacculated stones and those that are too large to lock the strongest lithotrites on will always have to be cut. But with these few exceptions every thing else should be crushed. Last, but not least, let us look at the dangers of the operator's own making. They begin with the selection of instruments and last until the evacuating catheter is withdrawn for the last time. There is not a step which is not fraught with danger to the patient and disaster to the operator, if he does not know what to do and how to do it gently. If he makes a false passage you know the result. If he catches the bladder between his blades, you are familiar with what follows. If one of his blades twist or break, a member of this body, I am told, can say how unpleasant it makes it for you. If, on the withdrawal of the crusher, you fail to free it of every thing but the small amount of sand that will lodge in the toe of the female blade, another member can say how disagreeable it is to drag a fragment out of the bladder and get it impacted in the urethra. If you fail to have on hand the most powerful lithotrite, another Kentucky physician can tell you how cheap one feels to have to cut for the nucleus after successfully evacuating the larger portion of its periphery. In the evacuation we must be as gentle in the working of the bulb as in the introduction of the large tube, and after it is safely in the bladder it must not be allowed repeatedly to suck a portion of the mucous membrane against its eye, for unpleasant reactions are apt to follow.

In answer to these objections, I would say, as Bigelow has so often repeated, it is next to impossible to get hold of a bladder that is distended, and that any surgeon is to blame who will use an untried lithotrite. I would not have one, he says, on which I could not exert my whole strength. If you will use the ordinary rules of lithotrity, I am sure that in an anesthetized patient you will never impact a fragment in his urethra. Finally, when are we sure we have removed the last particle? All I can say to this is, you feel confident when the catheter, which, by the way, is the best possible sound, fails to find any thing after pro-

longed search during the working of the bulb. Should a small portion be left after the bladder has been thoroughly distended, there is not the risk that there was when crushing was done with only three or four ounces of water in its cavity, for this distension of the organ produces a certain amount of temporary paresis, which prevents the organ from wounding itself on its rough angles, and thus saves the reaction. Besides the reaction, this method of removing stone saves intact the mouths of the ejaculatory ducts, the destruction of which in lithotomy has left so many to sterility.

So, in conclusion, we see that the greatest dangers are of the surgeon's own making, so that it behooves him not only to be careful in the selection of his instruments, but in every movement that he makes with them. No one should undertake this who is not personally familiar with all the other urethro-vesical disorders, and who is not already a tried surgeon, for in no position could an excited operator do more harm.

DANVILLE, KY.

A SUPPOSED ABDOMINAL TUMOR, WITH SLOUGHING OF THE CYST.*

BY JAMES S. PARRISH, M. D.

On May 22, 1878, I saw, with Drs. Ewin and Thomas, of Smith's Grove, the wife of Rev. Mr. J., age forty-three years; married twenty years, had never borne children, though delivered of what was supposed to be a false conception four months after marriage; menstruated at sixteen, regular since, though sometimes scant in quantity, and always accompanied by pain; has suffered occasionally from uterine prolapsus; bowels habitually constipated, and when acted upon by medicine the dejections give pain. She noticed an enlargement in the region of the right ovary about six or seven months before,

*Read before the Kentucky State Medical Society, 1884.

which increased until it reached about midway the abdomen. She now presents the appearance of a woman six or seven months pregnant, the uterus or tumor showing in the umbilical region slightly to the right of median line, solid, movable. Vaginal examination reveals the os high up, slightly retroverted, vaginal cul-de-sac shortened and indurated. We were unable to introduce uterine sound. The next day we again failed to do so, but succeeded in introducing a small probe to the depth of two or two and a half inches. Nothing seemed gained by the examination unless it served to show that the tumor was extra-uterine. The diagnosis remaining obscure, it was agreed to give some mere general treatment and wait. Ten days later I was telegraphed to come and aspirate the tumor. The patient was not so well as when first seen, but was hopeful and willing to undergo whatever was determined on. The next day I aspirated the growth with a large needle and drew off a small quantity of straw-colored fluid. The patient became suddenly nauseated, and syncope ensued. The operation was at once suspended and stimulants, etc., given. Severe abdominal pains occurred quickly, peritonitis followed; pulse rose to 120; temperature to 102°; tympanitis; great tenderness over whole abdomen, with persistent diarrhea. Patient rapidly lost flesh and strength, and for some days death seemed imminent. Toward the close of the second week, however, her condition improved, and seventeen days after the operation she was removed in a spring wagon to her home, distant about fourteen miles.

A week later her husband reported that the patient was taken with severe pains in the lower part of the abdomen, which sent her to the vessel, where she felt something give way, as she expressed it, and pass out of the rectum. On examination, the vessel was found to contain what, in the language of the husband, seemed to be "a mass of flesh." Dr. Hackney, a neighboring physician who had been called in, said the mass was about sixteen inches in length, nearly as broad, having an irregular border. A few days after I found the patient over her alarm. Improvement at once set in and she was soon entirely

well. She regained her flesh, and remains in good health to the present time.

The mass has never been examined microscopically, and I am still at a loss to know what is its real character.

GLASGOW JUNCTION, KY.

PHLYCTENULAR CONJUNCTIVITIS; TWO CASES.*

BY T. D. FINCK, M. D.

The two cases of phlyctenular conjunctivitis I am about to report both followed upon measles.

CASE I. Lester, a boy twelve years old, tall, slender, clear, pale complexion, had, in March, an attack of measles, for which I attended him. The eruption lasted four days. The eyes were not specially sensitive to the light, were not painful, and the conjunctiva showed but little irritation. The temperature was never very high. He got out of bed and at play in the house in ten days. A week later he was out of doors. A month after he complained of a burning, stinging pain, with a sense of pressure, about his eyes, and occasionally inordinate itching. I found increased lachrymation, the conjunctiva injected and thickened. There were nodules and little excoriations side by side, skirting the margin of the cornea. The tarsal conjunctiva was quite red and the Meibomian glands very prominent. The patient was more pale and emaciated than when dismissed a month before. The cervical glands were very much enlarged. There were large crusts about the nose. I directed small doses of calomel, quinine, and the syrup of iodide of iron. The crusts were removed by a solution of borax. In addition, he had a salt-water bath daily, to be followed by friction with a coarse towel, and to be in the open air as much as possible, and his eyes frequently bathed in tepid water. Until the fourth day there was no marked change, except the lachrymal secretion

*Read before the Kentucky State Medical Society, 1884.

was a little more catarrhal, and a few of the nodules had deliquesced. Soon after the glands of the neck began to diminish in size and the eyes to improve. No new nodules formed, and the old ones deliquesced, all being accomplished in about ten days.

CASE II. J., aged seven, short and stout. He, like the first case, had recovered from an attack of measles three weeks before. The eyes presented the same features. The glands of the neck were somewhat more enlarged, and none of the conjunctival nodules had deliquesced. The same treatment as in the first case brought about recovery in much the same time. None of the excoriations showed a tendency to extend into the deeper tissues.

The eye trouble in both cases was attributed to measles, and if with truth, they possess a certain interest, for the conjunctivitis which succeeds upon measles is, as is well known, ordinarily of the catarrhal kind. The pathology of phlyctenular conjunctivitis, as is also well known, consists in engorgement of the lymphatics of the conjunctival membrane, and subsequent rupture of one or more of them in the limbus of the conjunctiva.

I need hardly add that enlargement of the glands of the neck always co-exists with conjunctival herpes. The distribution of the lymphatics in the conjunctiva affords sufficient explanation of this, and also, I think, corroborates the view that phlyctenular conjunctivitis is due to glandular engorgement, while it excludes measles as a cause of the disease, other than in a general way, and through the system at large.

LOUISVILLE, KY.

KENTUCKY STATE MEDICAL SOCIETY.

Proceedings of its twenty-ninth annual session, held at Bowling Green, June, 1884:

MISTAKEN DIAGNOSIS.

Dr. D. W. Vandell, of Louisville, said:

The report read a moment ago by Dr. Stone* merely adds another to the list of mistakes in diagnosis. The valet of Beau Brummel, coming one morning from his master's room with a lot of crumpled neckties on his arm, remarked, "these are our failures." It is a pleasure to think, however, that with increase of knowledge our failures grow less and less frequent. There are few physicians who have seen much disease who can not refer to mistakes committed by themselves at least.

There is a gentleman here present through whose kindness I saw, four years ago, a case of enormous abdominal tumor. He himself thought the disease was ovarian, but a practiced ovariologist living in Philadelphia and a very distinguished general surgeon of that city and myself all held that the tumor was a fibroid. Time went on, and one night about a year ago the woman was seized with all the alarming symptoms which denote the bursting in the belly of an ovarian cyst. The medical man called to her tapped her, as affording the best means of relief, and drew off an enormous quantity of fluid. The patient rallied and got about again. The tumor refilled. I subsequently did ovariectomy, and removed a very large cyst, absolutely without adhesions.

I call to mind also the case of a widow, whose husband had been dead a year, who had an abdominal tumor. Two eminent gynecologists in Louisville and two in New York examined the case repeatedly, and all concurred in the opinion of the growth being a fibroid. Five months later she gave birth at full term to a child.

Some years back, a gentleman, who was then the most experienced and successful ovariologist in Louisville, pronounced a case ovarian disease and advised removal of the tumor. The surgeon whose case it was, and who of course concurred in the diagnosis, made every preparation for the work, even to fixing the day and selecting his assistants. Nature saved the surgeon any further trouble by bringing on labor a few hours before the time appointed for opening the abdomen.

* See page 73.

These and like cases simply show that very wise and experienced practitioners make mistakes in diagnosis. It unfortunately happens, however, that these mistakes are sometimes used as "a refuge for incompetency."

POPLITEAL ANEURISM.

Dr. T. J. Townsend, of Bowling Green, reported a case of this disease :

Frank Ewing, colored, aged sixty-two years, occupation, wood-sawyer, received on the 10th day of August, 1882, an injury on inner surface of left knee from a flying stick, which resulted in the speedy formation of a small tumor in the popliteal space. Its progressive increase of volume continued until the 3d of the following April (about eight months), when patient applied for treatment. On examination I found a pulsating tumor, the size of an ordinary coconut, occupying the entire popliteal space; a small spontaneous aneurism was also discovered four inches above. The leg was edematous, enormously swollen, and very painful, a condition produced by compression of popliteal vein and nerve; skin was smooth, with varnish-like surface.

Treatment by flexion or compression was considered inadvisable, and deligation of the femoral artery was resorted to as the best expedient, after which the leg became very cold, even as in death. To soothe the burning, stinging, pepper-like pains complained of and to encourage circulation in the injured member, the vigorous application of friction by the hands was suggested, and became for some days his only solace and comfort, for its long discontinuance never failed to produce the inevitable howl for its resumption. During intervals of rest or sleep the limb was enveloped in cotton wool. On the twenty-fourth day the ligature came away, and permanent cure was well pronounced. At the writing of this, fourteen months later, examination finds a hardened semi-elastic tumor at the seat of the large aneurism, the smaller one having spontaneously and entirely disappeared.

SHOULD HEMORRHOIDS BE OPERATED ON WHEN INFLAMED?

Prof. J. M. Mathews, of Louisville, addressing himself to this question, said :

From a variety of causes, piles are liable to become inflamed, and once inflamed, they may easily become strangulated by passage below the sphincter. Every thing is aggravated in this condition, and it may take some weeks to quiet the trouble. It has occurred to me, why

not operate upon and get rid of them at once? There is no authority that says "operate upon a pile during the inflamed state," but they will tell you to apply treatment to reduce the inflammation. I want to state one or two cases. A few weeks ago I was called to a lawyer who was in this condition. The family physician in attendance had tried in vain to quiet inflammatory action for two or three weeks. I found, hanging down from the anus, two solid tumors; I passed the knife around them and ligated them. I visited him the following morning, expecting to find him in some trouble. To my satisfaction, he was out of the house in one week's time. Another case: A young man had a mass of inflamed tumors hanging from him larger than my fist. It would have taken several weeks to abate the inflammatory trouble, and I ligated the whole mass. I went to see him the next morning. I was told by the people at the house that he had rested well all night, and got up early in the morning and went out. They sent for him, but he could not be found. Three days later I received a postal from Cairo, Ill., saying that he was that far on his way home, and was all right. When he got home he wrote me that he was entirely well. Since then I have had, I suppose, five or six cases of similar character, in which the proceedings and results were similar. I have therefore concluded that instead of applying remedies to relieve the inflammation in the tumors, they should be operated on at once.

THE COMMITTEE ON MATERIA MEDICA

Reported verbally through its chairman, Dr. J. P. Thomas, of Pembroke.

Dr. T. said he had made a fair test of the hypnotic, paraldehyde, suggested as a substitute for chloral; found it a very mild soporific in those cases of insomnia from excitement; that it seldom exercised an effect after two hours subsequent to its administration. Kairine as an antipyretic he had found to be quite inferior to salicylic acid and quinia, and only successful in dangerous doses. Its action upon the heart was out of proportion to its powers as a febrifuge. He had used hyoscyamin in but one case of acute mania, and instead of allaying it seemed rather to increase the excitement.

Dr. J. M. Mathews remarked:

The chairman has spoken of a substitute for chloral. I have used that article for years, and I must now confess that it is less a favorite with me than it formerly was.

Prof. J. W. Holland, of Louisville, added:

I wish the reporter had included the word opium in his reference to melancholia. More than a century ago Sydenham pointed out the cordial qualities of opium, and I fear we sometimes lose sight of that in our consideration and use of the drug as a hypnotic. If there be a melancholia, it proves more profound when chloral is used than when opium is employed, and I fear the same may be said of hyoscyamin.

Dr. Pinckney Thompson, of Henderson, hardly supposed that any one considered chloral to be a curative agent beyond the power which it exerts to control a spasm of pain. In gastric neuralgia in full doses it will often relieve promptly, and so in all those attacks of acute pain which do not tend to recur once they are relieved. We are all well enough aware that chloral and opium are poisons in poisonous doses, and they have to be given with circumspection.

Dr. Fuqua, of Hopkinsville, thought it was the province of the discreet physician to find the proper place to use chloral, and then to use it with the same rule in view which guides the use of other powerful agents—use it for its effect.

Dr. McCoy called attention to the excellent results chloral gives in cases of puerperal convulsions.

Dr. B. W. Stone remarked:

The hyoscyamin has been somewhat extensively used at our Institution (The Western Kentucky Lunatic Asylum), and we consider it, especially in acute mania, a most valuable hypnotic. Our doses are large, and invariably given hypodermically, one tenth and one twelfth of a grain being the ordinary dose. This quantity has been used night after night for several months, always inducing sleep, but with no amelioration of the mental condition. Its soporific effect is more decided than that of the fluid extract of hyoscyamin in proportionate doses. It is probably not often to be preferred to chloral hydrate, a well-tried remedy in the same class of cases. Neither medicine can be continuously used for a long period without deleterious effect upon the general mental condition. We use them now much less than formerly, and rarely continue their use above ten days or two weeks. Chloral and the bromides are sometimes advantageously combined.

The influence of morphia in *melancholia* is most excellent. The suicidal tendency is often allayed or eradicated by this drug; while

chloral, without doubt, occasionally arouses a dormant suicidal disposition in melancholics. I have in mind one patient who was cured of her melancholy by the medicine. This was one of two years' standing, in whom no evidence of improvement had been previously apparent. From one half to one grain was administered night and morning, with the effect of relieving her entirely in two weeks. She was discharged restored in one month, and has since remained well. No opium habit was induced as the medicine was gradually withdrawn.

In reply to a question asked of Dr. Stone as to

THE TREATMENT OF EPILEPSY

at the Lunatic Asylum, he replied:

We treat epilepsy almost altogether with the bromides of sodium, potassium, or ammonium. Our cures in fifteen years of extensive experience may be counted on the fingers of one hand. One epileptic man, very violent and homicidal after a series of paroxysms, had no seizures after commencing the medicine, during a period of four years' stay at the asylum. He was discharged as restored in mind, and returned two years subsequently, not having in the meantime had a single recurrence of trouble. I should mention that the cases of epilepsy which find their way into asylums are usually of the most obstinate type. The prescription in use at the institution has apparently restored half a dozen or more persons afflicted with the disease outside of the asylum.

THE PROGRESS OF SURGERY

Was reported on by Dr. L. S. McMurtry, of Danville. The first portion of the report was devoted to the subject of anesthetics. Recent studies as to the comparative merits of chloroform, sulphuric ether, bromide of ethyl, and anesthetic mixtures of these agents, were reviewed in detail. The experimental investigations of Dr. B. A. Watson, of Jersey City, were carefully considered, and his conclusions were that sulphuric ether is decidedly the safest anesthetic which has yet been employed in surgical practice, the mortality being relatively small when compared with that of chloroform, bromide of ethyl, and the mixtures employed. These agents of experimental study are confirmed by clinical experience. Dr. McMurtry doubts if any

agent will ever be found which can carry patients so near the border lines of life in safety. He doubts if any thing is accomplished in avoiding danger by the indiscriminate exhibition of alcohol before giving an anesthetic. He then detailed the method employed by Mollière, of administering the ether vapor per rectum, and mentioned the recent publications of Dr. William F. Bull, James B. Hunter, and Robert F. Weir, upon this mode of anesthesia. The brief experience already recorded, he claimed, demonstrates the danger of the method, as exemplified by intestinal hemorrhage or diarrhea; but the advantages of a method which suppresses the period of excitement, permits accurate regulation, avoids bronchial irritation, and aids materially in operations about the face, are apparent, and the method deserves to be tested thoroughly. In concluding this portion of the report the speaker commended the ether inhaler of Dr. Allis, of Philadelphia. The greater portion of Dr. McMurtry's paper was devoted to recent improvements in abdominal surgery. He stated the indications of treatment in contused, lacerated, punctured, and gun-shot wounds of the intestines to be prevention of fecal effusion, hemorrhage, and peritonitis. To effect these, the abdomen should be opened, bleeding arrested, the wound of the tube securely sutured, and the peritoneum thoroughly cleansed. In this connection he quoted the late Dr. Marion Sims, who wrote, in 1882: "I have the deepest conviction that there is no more danger of a man's dying of a gun-shot or other wound of the peritoneal cavity, properly treated, than there is of a woman's dying of an ovariectomy properly performed."

Dealing with another branch of abdominal surgery, Dr. McMurtry alluded to the difficulties and uncertainties which surround the diagnosis of abdominal tumors. These difficulties were so great that in a large number of cases the diagnosis can only be positive when the abdomen is opened. He detailed illustrative cases from his own experience. In one case—that of a young lady with a fluid accumulation in the abdomen—the patient had been in the hands of several skillful surgeons, with

a different diagnosis by each. After aspiration and the removal of two gallons of fluid, complete recovery ensued, thus showing the tumor to be a cyst of the parovarium.

Dr. A. W. Johnstone, of Danville, said :

There is one addition to the report that I think ought to be made with reference to the treatment of the wounded peritoneum, and that is drainage.

Lawson Tait speaks of drainage by the alimentary canal ; he says he has long been in the habit of purging the cases that need drainage, and has secured excellent results from the practice. We all know that when a purgative is given, water must come from some source. I myself have had some experience with this treatment, and the results were most satisfactory. There is another point which I consider of importance, and that is the size of the drainage-tube to be employed in ovariectomy. I am much pleased with the very large drainage-tubes now in use. In one case in which I operated the adhesions were so extensive and the bleeding points so numerous, after the tumor was severed from its attachments, that I found it absolutely necessary to employ the persulphate of iron to stop the hemorrhage. I expected trouble from it afterward, but I was much gratified to find the clots, iron and all, passing through the large tube I had inserted.

Dr. D. W. Yandell remarked :

With reference to the future of shot-wounds of the peritoneum, I dissent from the doctrine of Dr. Sims. I differed with my gifted friend before he had committed his views to paper. I differed with him when the subject came up between us in London, in 1881. I wondered much that a man of his experience in shot-wounds of the abdomen—for he had seen this class of injuries in the Franco-Prussian war—should have allowed himself to think for a moment that the dangers of wounds made by balls entering the abdomen were no greater than those arising from ovariectomy. The conditions involved in the two are certainly very dissimilar. Even if this were not as I have stated, there would still remain a very broad distinction between wounds inflicted by different missiles themselves. For instance, an ounce ball driven from a rifle through a man's belly inflicts a very different wound from that made by a ball from a Smith & Wesson's pistol. A wound from a Derringer is quite a different affair from one made by an ordinary six-shooter. I do not remember having

seen in the late war one man shot through the belly with a minnie ball, and who gave evidence of the ball having injured in its course a gut or a vessel, who recovered.

Wounds of the bladder inflicted by minnie balls were, according to my observation, invariably and quickly fatal; not, I think, always because of injury to the bladder, but because this character of projectile tears and bruises things dreadfully in its track. It is originally of such size, and is so often flattened and otherwise changed in shape in its course that it can scarcely penetrate the abdominal cavity without doing an amount of injury often entirely beyond the hand of the surgeon to repair. The ball from a Derringer pistol is of equal weight with a minnie, and at short range inflicts a wound but little less dangerous.

But I will not pursue this point further. If there be those here who subscribe to the teaching of Dr. Sims, they must still allow that there are shot-wounds and shot-wounds of the belly, and that the decision of operative interference in these injuries must depend largely on the size and nature of the projectile. Most men whom I have seen gut-shot were practically beyond the reach of surgery before surgical succor arrived.

I said to my friend, Dr. Sims, when we were discussing this question, that I did not think his doctrine expressed the real state of our resources. What Dr. Sims said was tantamount to saying that ninety to ninety-seven per cent of recoveries ought to occur after shot-wounds of the abdomen. This, I think, is calculated to mislead not only the profession but the public. I venture to say that taking the experience of individual members of this Society in shot-wounds of the belly—and this, as they live in Kentucky, will probably include half dozen cases each—five of the six will die, three of hemorrhage, one of peritonitis, and one of septicemia. More than twice as many will die of hemorrhage as of any thing else. Shot-wounds then in their very nature and from bleeding alone are more dangerous than ovariectomy, for hemorrhage of a kind that kills at once and beyond chance of arrest is not a feature of ovariectomy. The hemorrhage that occurs in ovariectomy the surgeon expects and is consequently prepared to meet. He knows as he proceeds when and whence it will come, and he arrests it on the instant. This is far from being so in shot-wounds. The precise course of a ball after it has once entered the body is beyond the ken of any surgeon I have met.

Yet I would not be understood as opposing such division of the abdominal walls as may be needed to make the search for wounded

gut or injured vessel easy and complete. For I am, in fact, one of the most pronounced advocates of this doctrine as taught by the lamented master whose recent death we all mourn. I beg you to understand it is not the doctrine which declares this to be the proper practice and the only practice to pursue that I oppose, but it is the declaration that this procedure has been made by our knowledge of antiseptics and drainage as safe as ovariectomy. There are few cases of ovarian disease which the surgeon would decline to remove—there are many cases of shot-wounds in which opening the belly would be considered quite beyond good surgery, at least when the surgeon reaches them. Given, that you should be on the spot at the moment the ball is received and be fully equipped for laparotomy, I should say that even then if the ball alone penetrated the cavity and were from an ordinary pistol it might still not be good surgery to cut into the belly in search of it, for ordinary pistol balls do neither always open intestines or tear vessels. They occasionally penetrate the cavity without inflicting any injury whatever.

But I set out to say but little more than that this dogma of that very great and very good man, Dr. Sims, published in a moment of enthusiasm, is misleading to a degree which makes it necessary for some one to say that it is not founded in fact, at least in such poor knowledge as we yet possess, and that shot-wounds of the abdomen, no matter by whom nor how treated, are far more dangerous than wounds made in the operation of ovariectomy.

Dr. Pinckney Thompson, of Henderson, read a paper on

TYPHOID FEVER,

from which we condense the following:

Typhoid fever is a disease that has a distinct clinical history, runs a distinct course, and in my judgment is never aborted by remedies. The two important conditions to consider are the heat, that necessarily develops—the consuming fire which, in both my judgment and experience, the majority of the patients succumb to—and the involvement of the heart in the way of partial paralysis. In making the diagnosis of typhoid fever, it is not necessary to regard disturbance of the alimentary canal as essential to establish the existence of the disease. In some cases of the most inveterate type I have found no such disturbance, but on the other hand a sluggishness amounting even to constipation.

In controlling the heat in this disease, there is nothing so efficient

in my experience as cold water. It need not be so cold as to produce shock—this is objectionable. I do not believe it necessary ever to have it below 75° or 76° , nor above 90° . It abstracts the heat, and should be repeated as often as necessity requires, as guided by the thermometer. I prefer the sponge bath, which is easily applied, not expensive, and in reach of every one.

The next important thing is to sustain the heart's action, and the best agent to accomplish this, and at the same time lower the temperature is, in my experience, alcohol. Another remedy I have found satisfactory in the early stages, in reducing the temperature and controlling the circulation, is salicylic acid in ten, fifteen, or twenty-grain doses.

Digitalis, in connection with mineral acids, as recommended by Flint, especially in connection with phosphoric acid, has given, in my hands, excellent results. I have also given the acids with strychnia. In the latter stages of the disease, by these, used in connection with belladonna, the heart's action is sustained, while at the same time the bowel troubles have been greatly mitigated. When, in spite of all, an excessive diarrhea with a tendency to hemorrhage supervenes, I know of nothing better than opium and subnitrate of bismuth.

An important feature of typhoid fever is the frequency with which intolerance on the part of the stomach manifests itself. I have seen cases, when ice and subnitrate of bismuth were promptly rejected, yield at once to iodine largely diluted in water, given every thirty to sixty minutes, as required.

There is no disease the physician is called to treat that demands from first to last a more rigid adherence to the principle of symptomatic treatment than typhoid fever.

Dr. John L. Taylor reported a case of typhoid fever in which for a time the temperature was subnormal, of which we make the following abstract:

A farmer, aged thirty-two years, had complained for a week with the usual prodromes of typhoid fever. I saw him March 30th, when I found tenderness on pressure and gurgling in the right iliac fossa; face purplish; tongue thickly furred and tremulous; temperature 103° ; pulse 90. The next day the patient was in mild stupor, and had passed a restless night. Bowels acted three times; temperature 103° —had risen to 104° the evening before.

During the three succeeding days condition unchanged. Skin and tongue now grew moist; stupor deepened; muttering; rose-colored spots on chest and abdomen. On the fifth day sordes appeared on

lips and teeth, and patient had five watery actions of ocher color. Other symptoms same. Next day delirium became more active; patient shouts at his horses, etc.; pulse dicrotic; epistaxis; involuntary stools; profuse perspiration; temperature and pulse rates same. Thirteen days after the temperature fell in the morning to 97° , and pulse to 68; lips and tongue dry; coma vigil, carphologia; subsultus, and involuntary dejections.

Three days later stupor still present, but patient easily aroused; skin and tongue moist; occasionally free perspiration; took temperature with five different thermometers. The several instruments each marked a temperature of 97° ; pulse 84. Virtually the same condition of things continued now for the next ten days. On April 22d the temperature rose to 99° A.M., 102° P.M., and so continued during the next thirty-six hours. It then fell to 98.5° , and so remained.

The treatment pursued in the case consisted in sponging the patient at short intervals, at first, with tepid water, to which a little brandy was added—later, while high temperature continued, cold water and the wet pack were used. Milk was almost the only food used, the patient taking from two to three pints daily, during the twenty-four hours. Turpentine, muriatic acid, opium, ammonia, quinine, brandy, and sulphuric acid were the drugs given.

The foregoing case, it will be observed, followed the usual course of typhoid fever, until the nineteenth day, when the temperature fell, without any appreciable reason, one degree and a fraction below normal, and thus continued for ten days. During that time all the other symptoms of the disease, except heat, were present in their full force. The heat then returned for thirty-six hours, the thermometer registering 103° A.M., 104° P.M. It then fell, and convalescence became steady. I need scarcely say that in all febrile disorders a marked and persistent decline of temperature is, as a rule, quickly followed by an improvement of other symptoms. The present case, however, is a marked exception, for every feature of the typhoid state continued for ten days after the temperature was sub-normal.

After the conclusion of Dr. Taylor's paper Prof. Wm. Bailey remarked:

I believe that typhoid fever is undoubtedly specific in character. The well-defined clinical history establishes this beyond doubt. I think that in all probability the essential cause is a specific germ, probably introduced into the alimentary canal with food and drink,

and possibly first entering the organism at the site of the glands in the ileum, which become the seat of the characteristic local lesion of the disease.

While I accept this as the most plausible theory of causation, not only in this, but in many other diseases, yet I am not ready, with my friend Prof. Holland, to confess that a complete demonstration has been made, as he claimed in his discourse on the "Cause of Phthisis." I shall at least wait for the inoculation of one more Greek.

To my mind causal relation has not been established between bacteria and the diseases in which they are found. I am waiting for the proof to be brought in, holding the question *sub judice*, in the meantime accepting the germ theory as most plausible, but I would not forget that it is but an hypothesis.

After the introduction of the specific poison we have a well-diffused thermometric history, which I need not repeat here. I ask your attention to the fact that but a very small part of the mortality in this disease is in any way due to the local lesion in the bowels, either through perforation, hemorrhage, or inflammation, etc., but we must look for the danger to our patients to approach from other directions.

Nothing is safer in medicine than to have a knowledge of the natural history of any given disease, so that, knowing the direction from which death will approach in making its attack upon the citadel of life, we may be forewarned and concentrate our forces for resistance.

I hold that the facts furnished by clinical observation in this disease prove that danger is to be anticipated in one way chiefly, after leaving out that small per cent already alluded to where death is due to local bowel lesion. In my judgment the prolonged elevation of temperature is the cause of the evil, and this gives us a valuable indication in the management of the disease. I have not much confidence in aborting or shortening typhoid fever by any known measures.

Having then a self-limited affection, whose course can not be cut short, we are shut up to methods of management rather than of cure. In perhaps ninety per cent of fatal cases death is due to asthenia, hence our aim should be to ascertain, if possible, the cause of the heart debility, and to manage the course of the disease so as to either obviate this or recover the patient from it when it has already occurred. As already stated, I think this asthenia is due in the large majority of cases almost always to the prolonged elevation of temperature. Other things being equal, we find the gravity of the case is in exact proportion to the range of the thermometer.

The evil effects of high temperature have been fully elaborated by Liebermeister, who has written the best article on typhoid fever known to me.

In this disease we have marked disturbance of nutrition, and as an effect particularly of the fever we have degeneration of tissue and especially of the muscle responsible for the circulation.

In the short time allotted me for this discussion I must be content to dismiss the consideration of danger from local lesion by the mere mention of the beneficial influence of ergotine in hemorrhage, and of turpentine in certain conditions of the ulcerated patches in the ileum in the third week of the disease.

I will limit my remarks to the means of preventing death by asthenia. Two factors are prominent in its production, failure of nutrition and fever. Indications are found then, first, for supporting the patient by a nutritious fluid diet; and for this purpose I know nothing better than milk at least partially digested by Fairchild's Extract of Pancreatine. The patient should have all that he can thoroughly appropriate, and of such food as leaves no crude material to pass down through the ileum. Any thing in the way of food, other in quantity or quality than so indicated, is an evil instead of good. I regard the measures instituted to cause the fever to run its course on a lower range of paramount importance. As we accomplish this, so we save disintegration of tissue, and avoid heart debility and likewise that well-known group of symptoms called typhoid, due no doubt to excessive metamorphosis and defective elimination.

Several different measures are in vogue for reducing temperature. I myself prize most highly the abstraction of heat by applications to the surface.

I would avoid shock by placing the patient in a tepid bath and then gradually adding cold water till the reduction is sufficient. This bath may be repeated as often as is necessary. Circumstances and conveniences will determine whether this shall be the method, or whether the pack or sponge bath shall be used. Sponging may be indulged in to almost any degree with benefit. Several remedies have been prescribed for their antipyretic influence in this disease, salicylate of soda, tincture digitalis, alcohol, and sulphate of quinine. After an experience of ten years with it, I am decidedly in favor of the quinine. I do not find an indiscriminate use of this remedy necessary in this disease. Some cases do not need it, and perhaps some may not tolerate it, but the latter class I have not met with.

Doctors are liable to deceive themselves in the use of this remedy

for this indication. Remember what I said just now, that we had a self-limited disease, and that this remedy is not given with the view of shortening the course one single day. We are so much in the habit of giving quinine to prevent the recurrence of fever that we are not liable to appreciate its influence when it is only antipyretic.

In order to accomplish this reduction of temperature, so desirable, we must have regard to the method of administration. I have had physicians say to me, that they had failed in reducing the temperature by quinine, although they had given thirty to sixty grains in twenty-four hours. Upon inquiry I found they had given it in five to ten-grain doses every three or four hours. I think such men have failed because they have not given the remedy in the right manner in order to secure its antipyretic influence.

Sixty grains given in the above manner will not reduce the temperature as much as one half the quantity will if properly administered. For this purpose we should very implicitly follow the directions given by Liebermeister and others. Give all you intend to give at a single dose, and that in solution. If necessary, I prepare the stomach for it by a small dose of morphia, one half hour before. I assure you that I have never given such a dose of quinine, for such a purpose, when I have afterward regretted it. As a rule, after giving a dose ranging from twenty to forty grains, the fever is promptly lessened, the patient gets a better night's sleep, and is in all conditions made more comfortable. The pulse is slower and the mental condition is improved.

The fever, as a rule, will not be high enough to require repetition for forty-eight hours. It should be given at about 8 P.M. I begin this treatment when the fever reaches its height, about the end of the first week, and continue it during the second week and into the third till we begin to get the morning remissions natural to the disease. So, as a rule, from four to six such doses will be all that will be required in this line. The water-treatment goes in hand with this and is greatly adjuvant. By this joint means I have saved my patient from a vast amount of tissue waste. I have prepared him, or, I should say, preserved him for a better and more satisfactory convalescence.

In conclusion, I beg leave to ask your attention to the necessity of efficient elimination, especially on the part of the kidneys, in order to relieve the blood of the effete products of disintegration, especially in the third week. In my hands in many cases alcohol has proved to be of service. Fever is no contra-indication to its use; on the other hand, alcohol lessens tissue waste and lowers temperature in fevers.

It has served me to call out in full the forces in the heart while it is in debility, and no doubt you may tide over a difficulty in cases where, without it, you could not. May not it also secure the heart a better blood-supply for its own nourishment, and hence, as has been remarked by another, prove to be "oats as well as spur."

Remember, our task is management, not cure; we nourish and preserve rather than medicate.

Prof. Larrabee said:

In both papers just read, no less than in the remarks of my friend, Dr. Bailey, it would appear that temperature, as indicated by the fever-thermometer, is the great indication, and to this one point remedies and treatment are to be chiefly directed. Dr. Thompson concludes his paper with a beautiful illustration of a ship in a storm, and as I am fond myself of nautical metaphor, I will take up the figure where he has left it. I use the thermometer in fevers very much as the sailor throws the lead, "leaning on the weather shrouds." I would not like to be without it in approaching a crisis or departing from the shore-line of health; it marks the soundings, and the soundings are corroborated by the chart in the binnacle; no one throws the lead in mid ocean. The daily increase in temperature, the evening exacerbation, in spite of treatment, for the first three or four days are quite infallible; but in the second or third week all is irregular, and the markings of the thermometer are to be taken together with all other symptoms.

I prefer to speak of the *management* rather than the treatment of a disease which is admitted by all to pursue a definite course, and the tendency of which is to recovery. I have at other times and in other places taken such decided ground in this matter that some of the members here present know already my views upon the *management* of typhoid fever and simple continued fevers. I believe that in not a few instances the poor patient (if you will excuse the expression) is deprived of a "fair show for his white alley" by hyper-medication, and that all cases are subjected to too much treatment. Nevertheless, there is not a disease known to us where so much can be done for the comfort and safety of the patient by correct sanitation and proper nursing. In the second paper read we have before us an array of figures denoting temperature and pulse, taken at least three times a day. We have little else than tests of temperature, in our local society meetings, as reports upon typhoid fever. I do not desire to lay any strictures upon the excellent paper before us; I only wish

to criticise what I have termed the modern practice. It would be a relief in these reports to find some mention of the disease itself, of the tongue with its sordes, the skin with its dryness or relaxation; of the rash and its relation to crisis and exacerbation days or weeks; of the gurgling in the iliac fossa; of the subsultus tendinæ, etc. I regard a twitching tendon oftentimes as significant as a rising thermometer.

There is such a thing as carelessness, and there is such a thing as too much care (over attention); the former is far more frequent than the latter. It is oftentimes in the "wee sma' hours," when tired nurses sleep and fires burn low that the spark of life goes out. But over-watchfulness amounting to ignorance, in my judgment, killed a President. The mortality among fever patients in wards of hospitals is much larger than in private practice. To be aroused every hour to swallow, now a draught of beef-tea, next a bowl of egg-nogg, next a drink of milk, next to have a tablespoonful of strong brandy poured down the throat; then, besides this, the regular medicine for temperature deviation, etc.; then to take the temperature and pulse, if kept up for twenty-one days, becomes of itself a large factor to fatal exhaustion.

Another, and, in my judgment, very great error, and one prolific of bad results, is the persistence with which the poor, fever-tossed patient is urged to take food against his inclination. Every two hours a pint cup of milk, egg-nogg, or beef-tea, until gallons are introduced, seems to be a most unnecessary embarrassment to a system which my friend, Dr. Bailey, has just said, is burdened with the products of metamorphoses of tissue. The mental aberration, delirium, and other symptoms of serious import are, no doubt, in a measure due to retention of effete matter in the circulation. The urine is scanty and high-colored. Can kidneys be aided by injection of more food? Shall we introduce more nitrogenous waste? Dilution, plenty of water to drink, becomes here the treatment instead of repletion. I am no longer afraid of starvation in typhoid fever, but I am afraid of *over-feeding*. Nine tenths of all cases in hospital and private practice are *over-fed*. I have seen typhoid fever patients vomit large casts or molds of milk, and pass the same from the bowels as large as a Bologna sausage after much pain, straining, and a risk of fatal hemorrhage. Milk should always, if given at all, be pancreatized and peptonized, and even then is not always the best diet for fever patients.

Dr. Thompson speaks favorably of alcohol for reducing temperature. The sum of our knowledge upon this point is the fact that

large doses of alcohol in disease will lessen temperature, and also that in many instances of blood-poisoning, its force expended in the system sustains life, or, rather, prevents death. Its use in typhoid fever should be limited, not so much to the reduction of temperature as to its necessity as a cerebral stimulant. And when so indicated as an arterial stimulant the high temperature present no longer, in my judgment, contra-indicates its use. It was formerly supposed that it added to the fever: this has been well proven to be a mistake. I can not, however, agree with Dr. Bailey in the tribute which he pays to alcohol, viz., that it is the "spur and the oats" together. This comprehensive expression may apply to digitalis and the congeneric drug, convallaria; but alcohol produces paresis of arterioles, by which more blood is brought to the surface, or skin, or cooling area of the body; this is not increasing but decreasing arterial tension, and weakening rather than strengthening the cardiac impulse.

Dr. Bailey advocates quinia in large doses, forty to sixty grains, for reduction of temperature in typhoid fever, after the plan laid down by Liebermeister. I have no doubt of the efficacy of large doses of quinine in controlling temperature; but if quinine is unnecessary in this disease for any other consideration, as I claim, it is certainly very objectionable in the face of other more simple and agreeable means, and the good possibly attained is more than counterbalanced by the evil effects upon the brain and nervous system, and muscular system also. There are other agents besides medicines which will reduce temperature. A blow on the head will reduce temperature. Many medicines act in the enormous doses named simply on the principle of *shock*, as the reaction and rising thermometer afterward show—small and frequently repeated doses of quinine, as Dr. Bailey tells us, fail to produce the required result. This shows that the brain and nervous system must be so profoundly affected by quinine as to prevent the reception of impressions before a marked fall in temperature occurs; and as soon as the quinine is eliminated, up goes the temperature again as high, if not higher, than before. It is well known that we can bring the fever temperature down to almost any desirable limit by the salicylates of soda, lime, etc., ten grains every hour, or twenty grains every two hours, are equal to a degree less temperature in the same space of time, but such treatment is open to the same objection: it is accomplished by shock, it depresses the heart to an equal extent, ruins the stomach for digesting food, and, moreover, produces no *pari passu* improvement in other disease symptoms. The disease ought to be taken into consideration along with temperature.

I am glad to hear Dr. Bailey speak of the tepid bath. I am a great advocate for the use of water externally, internally, and, I was about to say, eternally in fevers; but I mean warm water. I think some of us skipped the track a few years ago in adopting the cold and ice-water treatment; it was irrational. I do not, however, think, with Dr. Bailey, that the bath-tub is the best means of attaining the prolonged effect of water upon the skin. Often in the third week the patient is too weak to be moved about out of bed and in again. A plan, which I advocate in infants and children, I extend also to these cases, and it acts like a charm. Its effects are so continuous and it is so readily given in all places, the humblest abode furnishing all the materials as well as the palace, that it recommends itself on these considerations alone. My plan is to fold one or more sheets so as to correspond with the length of the patient's body from the neck to the toes. Have the patient stripped naked and these sheets folded out of steaming water and the patient snugly rolled up and rolled in a blanket on rubber cloth like a mummy; when this is done the sedative effect of water in the shape of warm vapor is felt by the irritated peripheral nerves; the capillaries are dilated; the patient almost always goes into a quiet, sleep-losing delirium, and from which he awakens refreshed. It may be renewed at the end of twelve hours or kept up continuously, as the temperature may indicate. In this way, with daily change of clothing, pure air, plenty of good water—and dilute muriatic acid added so as to be faintly perceptible in the water—I have carried typhoid-fever cases of more than ordinary severity through the entire course of the disease. And if the duration of the attack is not shortened, I have the satisfaction of knowing that my patient has had a comparatively pleasant journey, and will have a more rapid convalescence—less tedious and freer from relapses than those cases in which more heroic treatment is pursued.

The question is asked, if I do not use quinine at all in the treatment of typhoid fever; and I answer more decidedly, *no*, not if I know it to be such. I am on record, in a published work upon diagnosis by Dr. Wells, to the effect that I would use quinia as a valuable diagnostic agent, and this statement, quoted by the author from some remarks made several years since, I have no reason to change. It is quite impossible in our part of the country, where malarial diseases are more frequent than typhoid fever, to distinguish the symptoms sufficiently in the first few days to make a positive diagnosis. A few small doses of quinia, preceded as it always ought to be by an emetic or cathartic, or both, will suffice to break up a malarial attack.

If no permanent relief occurs from this treatment and the evening temperature continues to increase, and the patient gradually assumes the hebetude so peculiarly diagnostic, it is probable that we are dealing with typhoid fever, and the sooner we leave off the quinine the better it will be for the patient. I have no sort of patience with compound terms, such as typho-malarial fever, diphtheritic croup, and such like nonsense, invented to give cover to mistaken diagnosis. There is a specific fever called typhoid, and there is such a thing all over the world as malaria—they are distinct and separate poisons.

Of course I consider temperature important; but if the other means, and especially the warm wet pack and the fever coil be properly used, I have no fear that high temperature will be the door through which the life escapes in typhoid fever. I am not afraid of high temperature, so long as the skin circulation is good, the skin flushed with blood; but there is a condition of paleness with high temperature which is a forerunner of evil, and is in all probability produced by the irritation of the capillaries by debris, as in Bright's disease of the kidneys. The warm pack, a few drops of tincture belladonna, are better here than quinine or even digitalis, to which Dr. Thompson alludes in his paper. Yet let me say that I am very fond of this drug (digitalis). It is the only reliable heart tonic, and for an irritable condition of that organ has been properly styled the opium of the heart. Its administration must be based upon a thorough knowledge of its physiological action. I have seen it do harm, and I have no doubt hasten death, in typhoid fever in the third week, where the heart muscle has been considerably weakened and attenuated. In full doses in such instances it is apt to produce cardiac eclampsia, a condition of systolic action once supposed to be cumulative action of the drug; but in one or two-minim doses, watching its effect upon radial arterial tension, it is not only safe but effective.

Dr. Yandell said in substance :

A discussion on typhoid fever at this time is especially interesting to me, because, besides the very able way in which the subject has been handled, it affords me an opportunity to compare the treatment of the disease as pursued to-day by our busiest men and that followed twenty-five years ago. I myself had typhoid fever twenty-five years ago, and was treated by that sagacious and eminent practitioner, the late Prof. Lewis Rogers, a name the mention of which will, I am sure, stand with every member of this Society as a guarantee that I had, during my illness, all that was best in that day in the way of

treatment. I first thought my attack was malarial—it occurred in September—and I took forty grains of quinine, with the effect of increasing my headache and general discomfort. Dr. Rogers came, and, sharing my opinion, gave me yet sixty other grains of quinine, divided in three doses, to be taken at intervals of two hours. The fever went on. Rogers thought I stood a fair chance to die during the first week, from nausea. During the nine weeks succeeding, I was delirious. Those weeks remain to this day a total blank except in one particular, and that related to food. The recollection of my aversion to that is distinct. No other memory, during all that long period, survived. My nurse told me that I took food at no time without a remonstrance, that I not unfrequently flatly refused to have it, giving always as a reason that it was exceedingly distasteful to me.

I made the same objection to every kind of drink other than water. I got no medicine, but was required to take food—milk—at first every three hours, then every two hours. I had frozen champagne—frozen, because otherwise it nauseated me, at about the same intervals of time. Three weeks after the delirium subsided I stood on my feet for the first time. I have mentioned my own case in order that I might tell of my great aversion to food, and that I was absolutely required to take both it and wine throughout my entire attack.

I am just as sure that food was given in this case oftentimes to my hurt, as it was always given against my inclination. Yet this was the practice of the time. And I fear it is far too much the practice of the present day. Since Tanner's protracted fast I have been much less disposed than before to force food upon patients against their wishes. I have made some short fasts myself, in the hope of wearing out a rheumatism. My first attempt I continued for eleven days, taking in that time but three glasses of water a day. I felt no particular inconvenience from my abstinence. In the winter just past I fasted twenty-one days, also in a rheumatism, in which time I had not more than a quart of milk, which I took hot. I can not say that my disease was benefited in the least. But I was without appetite, was averse even to food, and if I got no good from fasting I at least escaped any of the inconveniences which might have ensued had I taken nourishment.

In the last year I have treated five cases of typhoid fever practically without food, that is, without food except when called for, or when delirium was present, and then only when it was not refused. One of these cases occurred last July, in the person of a young man. His case begun much as mine did. He got quinine, as I did. He had the

same aversion to food that I had. He was not a milk-drinker when well—the suggestion of milk when sick was distasteful to him. Daily I asked him if he wished food, and daily he answered “No.” He went seventeen days on two or three glasses of water each twenty-four hours. His bowels acted with some regularity. On the twelfth day he had a little diarrhea, which was checked by bismuth and chalk mixture. On the morning of the eighteenth day his pulse was somewhat weak, and I said, “You must have some food.” He replied that he had dreamed the night before of eating a broiled partridge. Partridges not being in season, he got a chicken soup instead. A day or two after his appetite returned, and he made a good recovery. He went through four weeks of fever without a single unpleasant symptom. I spare you the details of the remaining four cases. The abstinence in none of these was so protracted, but food was given to none until it was acceptable, and all went through their attack with seemingly less trouble than those who were plied with food. When food is given, I agree with one of the speakers that milk peptonized by Fairchild’s process is more digestible than plain milk. I have also found that the beef peptonoids of Reed & Carnrick are usually both an acceptable and easily assimilated food.

Touching the use of quinine in typhoid fever, Dr. Rogers and myself were thrown much together in 1865, when there was a great to-do, you may remember, about typho-malarial fever (the new fever, as it was then called). During that and the following year we made a great number of experiments in the endeavor to ascertain the true value of quinine in this disease. The results of our labors may be stated thus: At no time while the fever of typhoid or typho-malarial ran an even, steady course did quinine do any perceptible good. When the fever began to subside, or to show some disposition to become periodic, quinine was then of distinct and very great value. I confess always to a feeling of surprise when I hear gentlemen say they find no inconvenience from these enormous doses of quinine given to reduce temperature in this disease. My observation (except under the conditions named) has been almost the very reverse of this. Rogers gave me twenty grain-doses of quinine at intervals of two hours—that is, I got a dram of it within four hours—and yet words are poor to describe the increase in wretchedness which these large doses brought.

I remember to have seen a case, in consultation with the late Dr. Foree, in which the disease, in the person of a doctor, lasted twenty weeks. Now, if ever I knew a discerning and sagacious practitioner,

it was Dr. Foree. Time and time again the question as to the propriety of quinine in the case we were attending was discussed, especially when the temperature went inordinately high; but we withheld the drug till the fever began to abate and grow somewhat periodic. When, one day, it fell to 100° in the forenoon, and then rose to 102° in the evening, Foree said, with great positiveness, I remember, "Now is the time for quinine." I gave ten grains every two hours until the patient had forty grains. I am aware that it has been said that this is not so good a way as to give the same quantity in two doses, or in half the time; but it reduced the temperature, nevertheless, and kept it reduced from twelve to eighteen hours. But it rose just as before, and it was not for days after that quinine seemed to have acquired the power to prevent a recurrence of the fever, and this, I think, was not until the fever was itself ready to go. Certainly not until it had assumed distinct periodicity. This, in brief, is the experience of Rogers and Foree. My own coincides with theirs.

Quinine accomplishes in typhoid fever just what it does in septicemia, for instance. Certainly, I think, no more. It unquestionably possesses the power to reduce temperature, just as salicylic acid does, and as some other drugs do; but the moment you withhold it the temperature goes back to where it was, and sometimes beyond that.

I am of those who do not see in the temperature alone the spirit and essence of typhoid fever. I have seen many a case of this disease, where the heat was but little exalted, perish miserably of hemorrhage; other cases, in which the thermometer never denoted the danger point, die of diarrhea, and yet others again, in which the fever was never a prominent feature, succumb to cerebral complications. I am also one of those who believe that there are other and better means—because less disturbing—more effective means, of reducing temperature than quinine, or any other drug, properly so called. Water, applied to the surface, in my hands is a far better antipyretic than any thing whatever given by the mouth. Water hot, water tepid, water of ordinary temperature, water cold, effects reduction of temperature in a much more uniform and less disturbing way than any drug does. My father called attention to the external use of water in fevers in a prize essay forty-four years ago, and he used water most extensively externally in the treatment of almost every class of fevers up to the very close of his life. I, too, influenced by his teachings and governed by my own experience, have trusted to the same agent in the management of malarial, typhoid, and scarlet fever. I was glad to hear one of the gentlemen say that he used hot water. As a rule,

I prefer a wet sheet to a bath tub, and so, as a rule, I allow the patient to have the water brought to that temperature which is most grateful to him. In high temperatures following surgical operations, I can not believe that I am mistaken when I declare that I get more good from the external use of water than I do even from the largest doses of quinine.

The three points I have endeavored to present in my remarks are, first, that food is by no means so important as it is usually held to be in the management of typhoid fever, and may be dispensed with to advantage far longer than is commonly believed; next, that quinine possesses no curative powers whatever in this disease, and that the good it does is sometimes, at least, purchased at the expense of the convenience of the patient; and, finally, that water, properly used, is the best antipyretic known.

Prof. Holland, of Louisville, remarked:

The antipyretic treatment of typhoid fever was such a departure from the expectant plan that it required the sanction of great authority to induce any one to try it. It was founded upon the observation of the direct pathological results of continued high temperature, and came to us fortified by the statistics of the German hospitals where, in over six thousand cases, there was an average mortality of only five per cent as against twenty per cent by the old expectant plan. In estimating the value of antipyretics these remarkable figures count for a good deal more than the experience of one man in private practice upon a few cases. The chances of coming to an erroneous conclusion are lessened in proportion to the number of instances examined.

My personal experience inclines me to favor the use of antipyretics in those cases in which prolonged pyrexia of a high grade threatens to seriously affect the patient's chances.

In the City Hospital, during my term of service, if the temperature on the evening of the seventh or eighth day rises above 103° Fahrenheit, I look for grave results from the heat factor and endeavor systematically to reduce these to a minimum.

If the stomach bears it well, I give two fifteen-grain doses of quinine between six and seven o'clock A. M. This will usually keep the temperature down for twenty-four to forty-eight hours. When it rises to 103° in the evening again, the quinine is given the next morning as before. Sponging with tepid water is also employed in the afternoon.

With respect to diet I concur with Dr. Yandell in the opinion that

Dr. Tanner's fast helped to rob abstinence of some of its terrors. Patients are not benefited by foods forced on them in the way insisted on by some. As much milk or gruel as the patient takes kindly at proper intervals may be given. When he turns away from such foods, cold barley water or thin chicken-water may be offered as beverages to slake thirst.

These principles of treatment, modified to suit individual needs or special symptoms as they arise, have yielded good results in my practice. As for the big doses of quinine, they may have such drawbacks as attend the use of all drugs of positive value. Even in malarial fevers quinine is not an unmixed good. Fullness of the head, ringing of the ears, partial deafness, nervous agitation, and weakness are expected and not considered of any weight as an offset to the antiperiodic property. So with quinine in antipyretic doses in typhoid fever. The unpleasant effects are more than compensated by the lower temperature, the slower pulse, the easier respiration, the cleaner tongue, the clearer brain, and the more rapid convalescence. All these are of value even if the disease itself is not aborted, and this effect is not claimed for it by any one.

Dr. J. M. Harwood, of Shelbyville, said:

My experience with antipyretic doses of quinine in typhoid fever where the temperature runs high has been most satisfactory. I have never had cause to regret giving twenty-five to thirty-five grains at one time, and I have seldom found it necessary to repeat it in less than forty-eight hours. The patients almost invariably rest better under its influence. To make a patient *supremely unhappy*, give two or three ten-grain doses two to four hours apart.

Prof. J. W. Holland delivered a popular address on "The Cause of Consumption." Our space permits us to give but a brief abstract of his remarks:

There are doctors here who no doubt will shrug their shoulders at the word *cause*, but I now employ the term in the singular for reasons which will appear. To the old stock of knowledge concerning this disease there has recently been added a fact of great significance. This fact was discovered about two years ago, and in that short space of time it has received almost universal acceptance. It is interesting to observe the influence of this new fact upon what we previously knew of the disease which it concerns.

Among the conditions long supposed to affect the course and

fatality of the disease, climate has held a prominent place. It was thought to have much to do with consumption; that is an error. In Europe, where the countries have well-organized administrations and where the fatality statistics have been kept with the most sedulous care, the mortality lists show the disease to be about equally fatal in the tropics with that which prevails in the north. About one sixth of all deaths are due to consumption; about one third of all people who die of disease die of consumption. Altitude, however, has much to do with the rate of mortality from this disease. It is found that as you ascend the mortality grows less and less, until a point is reached where consumption is comparatively unknown. Is this due to climate? No. Because if you go north, where the isothermal line descends until it reaches the surface of the earth, there you will find consumption prevailing. It is said to be due to the greater purity of the air and its attenuation. Occupation has something to do with the mortality from the disease. Those who live indoors a greater part of their lives in an atmosphere tainted by respiration are more likely to suffer from the disease. Age and sex both exert a modifying influence upon it; the years from fifteen to forty-five are attended by the greatest mortality, and more women die of it than men.

There is no more common observation than that "consumption runs in a family," or in a side of a family. Some other features have been noted, but the significance of them has been disputed. Dr. Rush, in his writings, says the disease was unknown among the American Indians, and it was not mentioned in their legends and traditions until a comparatively recent period. Now they die as freely as the whites. Livingstone and Stanley state that the negroes of Central Africa do not have the disease.

There is one other fact which appears to have an extraordinary significance beyond all these others. In 1874 three Greek physicians inoculated a man with the disease. They had this rare opportunity. He was doomed to die, and they inoculated him with the sputum from diseased lungs. Great care was taken to eliminate all sources of doubt. There was no history of family susceptibility. About the third week after the inoculation the signs of consumption began to manifest themselves in his body. At the post-mortem examination tubercles were found in the apex of each lung, and some on the free surface of the liver.

These are remarkable facts, and they are capable of two interpretations. Some will say that when the white man came among the Indians, he brought his firewater and his other methods of dissipa-

tion, and his system underwent the same deterioration which renders the white susceptible. Others may say, with Dr. Formad, of Philadelphia, in reference to the Greek experiment, "The man was half dead anyhow." But after all is said, here is a disease of definite character, prevailing in all parts of the world, always the same, operating uniformly through all the varied conditions I have mentioned, it would seem reasonable to refer a constant effect to one unvarying cause, and all these others should then be regarded in the light of predisposing. The lungs of a consumptive always contain what we call tubercles. When these little tubercles are examined under the microscope, after having passed through certain manipulations, they are found to always possess one character; they are found to be filled with very minute rod-like bodies, which have been called the bacillus tuberculosis. Previous to two years ago it was difficult to define a tubercle in scientific terms. Now, the element which above all others distinguishes the tubercle is the presence of these little rod-like bodies. Let us look into these a little further—they are called the bacilli of tubercle. A young German doctor by the name of Koch began the study of the disease known as splenic fever, and found by a series of masterly experiments that it was in all cases due to a little rod-shaped body, which is represented here in this map. This little body has the power of reproducing itself by division indefinitely, when it is placed upon a fertile soil. It is not a bug; I state this because the detractors of Dr. Koch's theory have designated it "a bug theory." It is a plant which has the power of motion, and of indefinite multiplication. It has two effects, one is localized; but when it gains entrance to the general circulation of the blood it rapidly produces another set of phenomena. It can be raised in crops. Koch raised hundreds of crops, and every time that it developed he put it into an animal, and every time he put it into an animal susceptible to the disease that animal died. I mention that to show you the beginning of the man's career. He was invited to Berlin, when, after a series of discoveries, perhaps less important, he turned his attention at last to consumption. He worked at this subject for five years, and after he had settled it conclusively he announced it to the world. Since then the subject has been thoroughly discussed, and it has borne the fiercest light of criticism. These bodies found in the expectoration of consumptives, when cultivated in groups, were found to grow more potent and reproductive the further they were separated from the original germ, but in every instance they produced like phenomena.

How does this affect the old view? It would appear to satisfy the conditions I mentioned a while ago. It is important to state now that there are degrees of the power of communication. The parable of the sower illustrates what I would express. "Some of the seed fall by the wayside, some in the stony ground, whilst others again fall upon fertile soil and reproduce themselves many fold."

Dr. Holland continued his remarks until he developed the theory of the contagiousness of consumption, and emphasized the necessity of care on the part of people who suspect that they possess this fertile soil in placing themselves under circumstances where they are likely to become the recipients of this germ. He further showed why it was important that persons whose family history was suspicious should live out of doors as much as possible, avoid crowded cities and all places where the air is impure and probably contaminated, etc.

Andrew Seargent, M. D., of Hopkinsville, read the report on "Ethics," from which we abstract the following:

No question has been productive of more discussion during the past year than that of ethics. The adoption by the New York State Medical Society of a code of ethics in conflict with that of the American Medical Association has done much to draw the discussion away from the principle involved, and has made it rather a test of party or fellowship.

At the last meeting of the American Medical Association, the action of the New York State Society was almost unanimously condemned, and the position taken at St. Paul and Cleveland has been indorsed by a large majority of the profession in Kentucky.

In the State of New York there is a large class of men in the regular profession who believe that in a science at once so uncertain and so progressive as medicine it is neither necessary or wise to control the conduct of any physician when human suffering is to be relieved. They believe that members of the medical profession should hold themselves ever ready to relieve a suffering public, without any limitation in the choice of remedies, or in the mode of their administration, except such as are imposed by an enlightened judgment and high regard for the interest of the sick.

To say that a desire to consult with homeopaths, and thus gain a few paltry dollars, produced the changes in the New York code, is to charge dishonesty and insincerity against a most numerous, intelligent, and respectable body of medical men. They believe that the prohibitory clause of the old code was adopted, not for the cure of

disease and the relief of suffering so much as to proscribe homeopathy, which was then in its infancy in this country, and claim the clause to be a failure. They believe that while they have been practicing the best system of medicine known, homeopathy has largely increased. Is it, then, astonishing that a large number of the regular profession should desire a change of tactics?

For myself, I do not favor the abolition of all codes because I have lost faith in the principles of the old code, but because the latter has failed to accomplish the purposes for which it was framed. Many will say that the principles embodied in the old code are too good to be laid aside. I answer that those principles will be just as binding on the conscience as ever. The principles of the old code will endure just as surely as knowledge and truth will endure.

The ideal code, presumably, would be one not of restraint, but rather one of guidance; simply a set of rules by which the conduct of physicians toward each other in the delicate relations of practice might be indicated, and which could be used for reference in doubtful cases. But better than any code is the unwritten and indefinable law which always governs the gentleman.

If physicians were absolved from even the ordinary obligations of gentlemen, and only required to observe the code of ethics of the American Medical Association, the profession of medicine would soon, I believe, cease to be the honorable calling that it is.

Yet I am not in favor of the New York code as it now stands. I regard it as a sort of compromise with half-acknowledged fraud. It abandons the higher law of the old code, and steps down to accept questionable legal enactments as authoritative. It recognizes the legally-qualified practitioner as our equal, without regard to his character.

The abolition of all codes is to my mind certainly an honorable path to harmony. It is at least the only "olive-branch" that we can now afford to extend to the irregulars. With no code, we offer no other antagonism than that of true merit, and sooner or later truth must prevail over error.

Meanwhile, as practicing physicians let us do all in our power to uphold the banner of truth and science. Let us leave therapeutic dogmas to unworthy rivals, and strive for whatever is good in the field of rational medicine, accepting from the hand of Experience such fruit as she may pluck from the "Tree of Knowledge," and give this to our patients as the best we can offer them so long as the "Tree of Life" shall remain inaccessible to human approach.

Clinic of the Month.

THE MORE RECENT IMPROVEMENTS IN LITHOTRITY.—The following is an abstract of a lecture delivered at the Royal College of Surgeons by Sir Henry Thompson :

" Five years of experience in lithotrity, completed at a single sitting, enables me to testify as fully concerning its capabilities, perhaps, as any other operator. It has been only partially tested at present on the continent; entertained very cautiously at Paris; but received with gradually increasing favor at Vienna, where it is now the rule; but it is not in either place accepted for the largest calculi, and a cutting operation is preferred for those hard stones which weigh one ounce and a half and upward in size. I propose, first, briefly to describe that proceeding which has appeared to me the most desirable in order to achieve successful lithotrity at a single sitting.

" In the first place, in respect of all instruments, we must recognize the importance of not inflicting any needless injury on the urethra and bladder, and should, therefore, always select the smallest lithotrites and evacuators which possess strength and capability adequate to crush and remove the individual calculus to be dealt with. And inasmuch as the great majority of calculi are small when first found by the surgeon, undue stretching of the natural caliber of the urethra is unnecessary for their removal, since no instrument need be used which exceeds the limit of that caliber. It is only when dealing with calculi of exceptional size that instruments which distend the urethra to a considerable degree are to be employed.

" Secondly, in dealing with a calculus of any size, but especially when large, it is important to employ instruments which are simple in construction and not easily liable to be deranged in action when employed in the bladder. The presence of débris

in quantity, mixed with viscid mucus, perhaps with some coagulating blood, has to be reckoned on in circumstances which demand prolonged manipulation; and mechanical arrangements which work admirably in experiment with limpid fluids outside the bladder are not unlikely to fail when brought to the test of actual service. Simplicity in the construction of instruments is, therefore, to be attained if possible.

"Let me suppose that the presence of a calculus has been determined, by sounding, in the bladder of an elderly male. It is almost certain that the note obtained and the resistance felt in making contact generally suffice also to convey an idea, approximately accurate, regarding the size and nature of the stone. As a rule, it is not so necessary now as it formerly was to make an exact diagnosis in relation to those particulars. When it was desirable to limit the application of lithotripsy to calculi of, say one inch and a half diameter, it was important to ascertain by some easily-managed method of measuring, what were their dimensions in every case. But now that the scope of lithotripsy is considerably increased, all that is requisite to be learned is, whether the stone is an exceptionally large and hard one, and whether any unnatural narrowing of the canal is present; and both of these points are almost sufficiently determined by an ordinary sounding in practiced hands. At all events it is unnecessary to make any further examination for the purpose of diagnosis until the patient has been placed under the influence of an anesthetic upon the operating-table, the surgeon having full liberty to exercise his judgment and act as circumstances demand.

"Invariably, as soon as the patient is thus prepared for operation, I pass a full-sized conical steel-plated sound—say about No. 15 (English), in order to determine that most important point of urethral caliber before touching the stone. If the urethra is healthy, and of course in the very great majority of cases it is so, the instrument has passed with perfect ease, and may probably be followed by No. 16 or 17 in an elderly adult, by which term I mean a man of sixty years and upward. I am

sure the urethra is more capacious in such patients than in those whose age is between thirty and fifty years. In these latter the urethra often resents any active distension, and the bladder also more readily becomes inflamed than in older patients. With a urethra admitting the easy passing of a No. 16 almost any thing is possible, and suffices for almost any calculus; but if I am dealing with one which weighs, in my judgment, about two ounces and more, I am glad to use larger sizes, and No. 18 I have never found it necessary to exceed. But if the stone is known to be small, No. 14 amply suffices, and there can be no reason for going beyond it. This is the size which was mostly used with the original instrument of Clover in 1866, and for all ordinary purposes answers admirably.

"The lithotrite is next introduced. With small and medium-sized stones, a light instrument of the half-fenestrated pattern—that is, the lower portion of the male blade penetrating the female blade, which prevents the possibility of blocking by débris, while the upper portion is flat and crushes the stone into small fragments—is, I think, the best. This mode of construction is applicable for a wide range of size in calculus, answering for any one of uric acid below about an inch and a half in length. To accomplish the first fracture of such a one, or to crush into fragments a larger calculus, a fully fenestrated lithotrite is better. There are some old forms of blade which have been in use for fifty years, which can not, I think, be excelled; so numerous are the patterns that have been employed, that it is difficult now to imagine a new design.

"To proceed: supposing that a considerable quantity of débris has been made, the calculus being large, it is advisable to withdraw the lithotrite, to introduce an evacuating-sound and attach the aspirator, so as to withdraw the débris already made before crushing more. When this is removed the same or a lighter instrument may crush the fragments into débris, before again using the evacuator. By this time, probably, the remaining portion is not considerable, and another introduction of the lithotrite, followed by another aspiration, very likely empties the

bladder. If, however, a fragment or two are heard and felt to strike the end of the evacuator, yet do not pass through it, they are probably just too large to issue, and require another crushing before the task is completed. There is certainly now no difficulty in removing any last fragment from the bladder, the more powerful aspirators emptying the cavity so efficiently that no other instrument is necessary for the purpose; while by no sound is the last fragment found so well as by the evacuating catheter, against which contact of the fragment is certain to be felt or heard, through the action of the outrushing current of water.

"In regard to aspirators, the original instrument of Clover, simple in the extreme, is a very good instrument, and requires only some modification to be as efficient as most of the modern ones. For my own aspirator I have provided a tap with funnel-shaped opening to the upper part of the india-rubber ball, by which to fill it and to remove air accidentally introduced, which offers a decided advantage. Last year I attached to the end of the evacuator, in the interior of the glass receiver, a light wire valve, which, while it admits any fragments to enter, renders their escape impossible; its action meantime being always visible to the operator.

"After the operation the patient remains in bed, and rarely requires any special management, except sometimes that which a simple traumatic cystitis demands—namely, absolute rest in bed; occasionally the urine neutralized by potash; hot hip baths; mild diet, and watchful care that chronic retention of urine does not occur, or is not permitted to continue unrelieved; avoiding, on the other hand, undue or unnecessary interference by means of catheters. But of all the agents for rapidly removing subacute or chronic cystitis after lithotripsy, nothing equals mild injections of nitrate of silver, the effect of which is sometimes almost instantaneous. I use only half a grain of the salt in four ounces of warm water at first, gradually increasing to a solution three times that strength, beyond which it is rarely necessary to go. Usually one application a day, sometimes two,

will, in the course of three or four days, remove the painful symptom and produce clear and healthy urine.

"My own experience of the system of treating all calculi but the very largest by a single sitting of lithotripsy dates from shortly before Christmas, 1878; the first example being a case which I saw with Sir Spencer Wells, who was present at the operation. Since that time I have adopted the system of completing the operation at a single sitting for every case in which lithotripsy appeared to be possible, with two exceptions only, to be described immediately. The total number of adult male patients upon whom I have operated during the period referred to, a term of rather more than five years, is two hundred and eleven. Of these, fifteen have been by lithotomy, and of course forming the largest and worst cases, with seven deaths. There remain, therefore, one hundred and ninety-six cases treated by lithotripsy. One of these was a Portuguese gentleman, to whom Mr. Clover thought it prudent to administer chloroform only for a very limited time. He had five sittings to remove a stone weighing no less than seven hundred and eighty-seven grains, and made a good recovery. The other exception was that of a large oxalate of lime calculus, which at that time I preferred to crush in four sittings. The débris weighs six hundred and forty grains. He is now perfectly well. Every one of the remaining cases, one hundred and ninety-four in number, has been treated at a single sitting, making a total of one hundred and ninety-six cases of lithotripsy at a single sitting, with ten deaths, or just five per cent. Total of two hundred and eleven cases of adults by the two operations, with seventeen deaths, or eight per cent. The mean age of these patients was over sixty years. And this is a result which hitherto has, I need hardly say, not been approached by any other mode of treating calculus in the adult. This subject, however, will be illustrated more fully in the succeeding lecture, when I shall present the results of my entire series, embracing upward of eight hundred cases by the two operations.

"I have crushed several stones of uric acid, and one of oxal-

ate of lime, considerably upward of an ounce in weight. The largest uric-acid calculus I removed at a single sitting weighed two ounces and three quarters, and occupied me seventy minutes. The patient was seventy years of age, and made a capital recovery. I can not speak too highly of the results thus attained. Indeed, nothing need be added to the figures I have adduced above, which exceeded any expectation I could have previously formed as to the success of operating on men upward of sixty years of age, with stones of all sizes. In conclusion, I think we must admit that the operation of lithotrity, at a single sitting, bids fair to supersede lithotomy for the adult calculous patient in all cases except those in which the stone is of rare and exceptional size."

ACTION OF DRUGS ON SECRETION OF MILK.—In observations recently made by M. Strumpf of the milk of goats and of the human female iodide of potassium was found to lead to a marked fall in the quantity of the fluid secreted; the proportion of protein and saccharine principles was increased while the proportion of fat was diminished. The quantity of iodine secreted was very small, so that the notion that iodide of potassium can be administered to children by way of their nurse is not sustained. Alcohol increased the richness of milk in fats, while the proportion of albuminoids and carbo-hydrates was not modified. Unadulterated alcohol was not detected in the milk. Neither alcohol, morphia, nor the preparations of lead had any influence on the quantity of the secretion. Salicylic acid seemed to excite secretion a little; pilocarpin exercised no effect in this direction. The richness of the milk in sugar was increased by salicylic acid, which passes out of the milk secretion in greater quantities in the human female than in the herbivora. Traces of lead were also recognizable in the milk of those subjects who were ingesting the preparations of lead. (The Druggists Circular.)

CIMICIFUGA IN EPILEPTIFORM NIGHTMARE.—Dr. Small reports a case almost identical with one published recently in the report

of Prof. H. C. Wood's clinic. The disease was epileptiform nightmare, and was cured by half a teaspoonful of powdered *cimicifuga racemosa* administered at bedtime. The paroxysms at once ceased, and there had been no return in a lengthy period of observation. The patient also was ordered to partake of only a light supper each night. (Medical Times.)

[When it may be asked, was it the drug or the abstinence? Ed. A. P.]

GUN-SHOT WOUND OF THE STOMACH—SUCCESSFUL LAPAROTOMY.—Professor Kocher, of Berne, has recently operated with success on a case of gun-shot wound of the stomach. A boy, aged fourteen, was admitted into hospital half an hour after having received a wound in the region of the stomach, from a pistol-shot aimed at him from a distance of about five paces. He was pale, and complained of abdominal pain; the abdomen was swollen, and distinctly dull on percussion inferiorly. Pressure on the abdomen caused pain. A quarter of an hour later, hic-cough, severe epigastric pain, vomiting, pallor, and symptoms of collapse came on. There was tympanitic resonance from the ensiform cartilage to the umbilicus, with complete dullness from the navel downward and in the flanks; the lightest percussion caused severe pain. Three hours after the injury laparotomy was performed. On opening the abdominal cavity in the region of the navel, a great quantity of dark blood escaped. The bullet-wound was discovered with comparative ease; it was situated on the anterior surface of the stomach, toward the greater curvature in the direction of the fundus. The wound was circular, with sharp edges, and about half an inch in diameter. The bullet could not be found, nor was there any aperture of exit. The edges of the wound were united, first with two catgut ligatures, like an ordinary wound, and then a continuous silk suture was applied for the distance of about an inch, so as to invert the serous coat around the wound. Recovery was retarded by an abscess which formed in the track of the sutures in the abdominal wound. Professor Kocher declares that, considering the

impossibility of recovery in cases of gun-shot wound of the stomach when active measures are not taken, it is the duty of the surgeon to perform laparotomy whenever an injury of that kind is suspected. He publishes several other recent cases of operations on the stomach. Out of three cases of resection of the pylorus, one recovered; a case of gastro-enterostomy, for cancer of the stomach, terminated fatally; one case recovered where the stomach of an agricultural laborer was opened for removal of the end of a coin-catcher, which had broken off during an attempt to extract a large tin nail which the patient had swallowed. The nail itself could not be found. Professor Kocher has also recently performed three gastrostomies for the relief of cancerous stricture of the esophagus. One of the patients died within twelve hours, from collapse; the second died on the third day with septic symptoms. The third recovered, left the hospital, and died suddenly two months after his discharge, apparently from an apoplectic stroke. (British Medical Journal.)

EXTIRPATION OF THE LARYNX AND THYROID GLAND.—Dr. Novaro performed this operation in Turin, on account of epithelioma involving the larynx, thyroid body, and a part of the wall of the pharynx, the latter being also excised. The patient died of pneumonia a month after the operation. (*Gaz. degli Ospitali.*)

Notes and Queries.

KENTUCKY STATE MEDICAL SOCIETY.—The annual meeting of this Society, which occurred at Bowling Green in June, was a good meeting. There have been meetings more numerous attended, but the workers of the Society were present in considerable numbers. The papers read were, with few exceptions, not over-long, while the discussion they elicited was general and spirited. It brought out much individual bedside experience, the only experience which finds a ready ear nowadays. The rehash of well-known opinions and quotations from the horn-books are received with rapidly diminishing interest, while the demand for the daily observation and clinical experience of the practitioner is steadily on the increase. "The old order changeth." The time is not far distant when pointed discussion, brief narrative of bedside experience, accurate statement of individual observation and a brisk interchange of work and opinions will supersede the tiresome essays of yore. And when that time comes we will all look forward to the meetings of our societies as to seasons of refreshing pleasure.

We give up a very large part of the present number of the PRACTITIONER to the scientific proceedings of the Kentucky Society. The next meeting of the Society will be held at Crab Orchard.

The following officers were elected: President, Dr. Pinckney Thompson, of Henderson; Senior Vice-President, Dr. O. D. Todd, of Eminence; Junior Vice-President, Dr. L. S. McMurry, of Danville; Secretary, Dr. S. M. Letcher, of Richmond; Assistant Secretary, Dr. F. Dunlap, of Danville; Treasurer, Dr. Edward Alcorn, of Hustonville; Board of Censors, Dr. Dudley S. Reynolds, of Louisville, chairman; Dr. James H. Letcher, of Henderson, secretary; Dr. A. M. Seargent, of Hopkinsville; Dr.

R. F. Floyd, of Lebanon; Dr. J. M. Harwood, of Shelbyville; Dr. R. C. McCord, of Lebanon.

PROF. JOHN A. OCTERLONY, M. D.—When Dr. Parvin removed to Philadelphia Dr. Octerlony succeeded to his position as one of the editors of *THE AMERICAN PRACTITIONER*. Our readers can bear testimony to the able manner in which he discharged the duties of the place. But the demands of a large practice added to the work incident to the chair he holds in the University of Louisville have obliged him to dissolve his connection with the journal. His pen, however, will continue to be used in its service. Books for review and communications in his particular line of work may be sent directly to his address, Fourth Avenue, Louisville, Ky.

THE ATLANTA MEDICAL AND SURGICAL JOURNAL.—This sterling publication comes to us in a new and handsome dress, and full of interesting matter. Its founder, Professor W. F. Westmoreland, after having withdrawn for some years from its editorial charge, has again assumed his place at the helm, having, as his associates Dr. H. V. M. Miller and Dr. James A. Gray. The Atlanta Journal is one of the best of our many exchanges, and we gladly welcome the return of its editor-in-chief, Dr. Westmoreland, to a position which he adorned for so many years.

THE TRAINING OF A MEDICINE-MAN.—The medicine-man among the Indians of French Guiana, who is called the *piaye*, is priest, doctor, wizard, and mountebank, chiefly the last, all in one. He prepares himself for his office by going through a course of special training, full of terrible experiences, to which he submits willingly for the sake of the advantages he expects to gain. The candidate, who is supposed to have had some kind of a call to the office, must obligate himself to submit, without flinching, to all the processes of discipline that are to be imposed upon him. Except for a little instruction in the

concoction of poisons, the discipline has no reference to the medical art. For six months he is put upon a diet of manioc, which he must feed himself with his feet, using his hands only to guide his feet to his mouth; then he is allowed dried fish, to be taken in the same way, and tobacco, of which he must swallow the juice. Having survived this for a year, he is "examined" by being held under water until he is almost strangled, and then made immediately to walk over red-hot coals, deliberately. Another year of the former regimen is given him to prepare for his second examination, when he is tied up in a bag full of red ants, previously well shaken to a pitch of savage excitement. He is next treated to a most ingeniously devised application of wasp-stings, and to a trial of snake-bites, against which he is permitted to fortify himself with antidotes. He may also be hung to a flexible rod by hooks stuck in his ribs, or by his thumbs and toes, and kept awake for a week at a time. After this course, he is permitted to assist his master by beating the drum around the sick man's hammock, and howling to drive away the evil spirits. His final trial is the drinking of a decoction of carrion and tobacco-juice, after which he is regarded as fully qualified to work upon the fears of the tribe, and extort from them all the service and tithes and tribute, and levy all the black-mail his victims can be forced to pay. As for medical treatment, there is none of it, not even the herb-doctoring; and this constitutes the chief advantage of the system. (Popular Science Monthly.)

WHY WE WALK IN CIRCLES.—The reason that, when lost or not able to see, we walk in a circle, is still undetermined. Mr. George H. Darwin believes that it is because we are right or left legged, our "leggedness" being generally the converse of our "handedness," and that therefore right-handed men, being left-legged, are most apt to deviate to the right, and left-handed men to the left. Himself and Mr. Galton and others, making personal experiments in walking blindfolded, found themselves describing circles not more than fifty yards in diame-

ter, to the right. Of eight school-boys, six, who were totally right-handed, strode longer from left to right than from right to left; hopped on the left leg, and rose in jumping from that leg; one boy pursued the opposite course; and the last walked irregularly, with no average difference between his strides. Walking on a match for straightness, the left-legged boys all diverged to the right, the seventh boy to the left; the eighth won the prize. Measurements of Mr. Darwin's own stride, and of the strides of his friends, showed the same connection between divergence and comparative length of stride. Mr. Thomas Hawksley believes that the reason for the divergence is to be found in differences in the length of the legs, not enough to affect the visible step, but sufficient to reveal itself in a considerable walk. (*Popular Science Monthly*.)

THE ITALIAN MEDICAL REVIVAL.—An interesting account is given by the *Lancet* of the present and rapid growth of medical science in Italy. Two new medical journals have just been started, which makes the total number of Italian medical journals fifty-four. Twelve of these are published in Naples, and ten in Milan. Milan is now a great publishing center. There are 1,200 students at the University at Naples, and the other universities at Turin, Pavia, and Padua are flourishing. The number of original workers is already considerable, and the names of Bizzozzero, Semmola, Bottini Bufalini, and Crudeli are familiar to American readers. It seems likely that the Italian school will revive the fame of its older days.

INSANITY AS A DEFENSE FOR CRIME.—G. B. Corkhill (*Medico-Legal Journal*), in an address before the New York Medico-Legal Society on "Insanity as a Defense for Crime," favors the plan, now adopted in Wisconsin, of having the special plea of insanity tried by a jury drawn for the purpose before the trial for the crime charged. This mainly on the ground that the prisoner, acquitted on the plea of insanity, might be found soon after "cured" or "harmless," and again turned loose on society;

while, if found insane by the special jury (though G. B. C. does not mention this part of the plan), the law should provide that he be guarded and restrained as a criminal lunatic, and discharged only after the closest examination.

EXCISION OF THE PATELLA.—Mr. Dodd reports, in the *Lancet*, an example of excision of the patella for caries, in which the joint was freely opened, and recovery ensued with a movable articulation. Instead of being the first recorded case of excision of the patella with preservation of the functions of the joint, as Mr. Dodd thinks, Dr. O. B. Knobe, of St. Joseph, Missouri, removed the bone for necrosis, with a similar result, and full details of the case may be found in the *North American Medico-Chirurgical Review*.

DR. THOMAS J. GRIFFITHS.—The death of this gentleman, which occurred a few weeks ago, will be deplored by a very large circle of friends. Dr. Griffiths was a well-known character in Louisville. He was a strikingly active and useful man. As a practitioner of medicine he was quick, judicious, discerning and industrious. As a man, he was independent, conscientious, and brave. As a friend, he was true as steel. He leaves a wife and one son, himself a physician, who gives promise of great usefulness.

TREPHINING FOR A BULLET.—An interesting surgical demonstration was given by Dr. William F. Fluhrer recently in the amphitheater of Bellevue Hospital, a patient being shown upon whom Dr. Fluhrer had performed successfully the operation of removing a pistol-ball from the brain through a counter-opening in the skull.

A DISTINGUISHED chemist once remarked to Sir William Jenner, "Do not forget that a pint of milk contains as much solid animal matter as a full-sized mutton-chop."

THE September number of the *AMERICAN PRACTITIONER* will contain a very large part of the scientific work done at the meeting of the Indiana State Medical Society, at Indianapolis, Ind.